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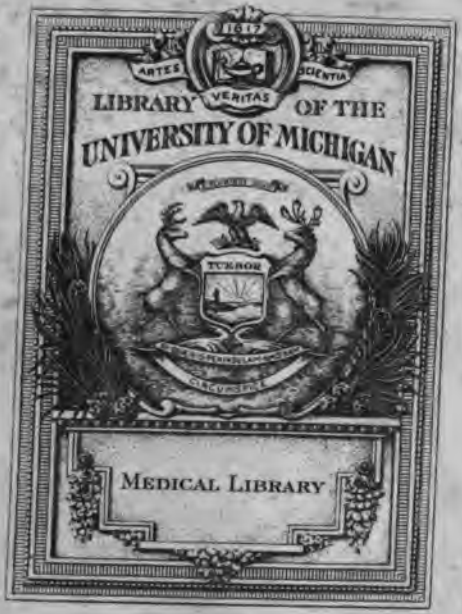

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OF THE

Royal ACADEMY OF MEDICINE

IN IRELAND.

VOL. III.

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RULES.

1. The name shall be, "ACADEMY OF MEDICINE IN IRELAND."

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, the four Presidents of Sections, the General Secretary, the Treasurer, four Secretaries of Sections, and eight Councillors, being two representatives from each Sectional Council.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Surgical, Medical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.*

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next; one black bean in four to reject.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communi-

* Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

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cations, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion; five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or division if demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting.

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Sectional Councils.

20. There shall be four Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Surgical, the Medical, the Obstetrical, and the Pathological, Council.

21. No Fellow shall be eligible as a candidate for election on more than one Sectional Council.

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section.

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the Obstetrical and Pathological Sections shall be elected by the Fellows at the Annual General Meeting in October.

29. (1.) One General Secretary to be elected at the Annual General Meeting.

(2.) It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the general Secretary in consideration of the fact that the editing of the Transactions is part of his duties.

30. One General Treasurer to be elected at the Annual General Meeting.

31. One Honorary Secretary for Foreign Correspondence to be appointed by the Council.

32. The Councillors for each Section to be elected at the Annual General Meeting in October. Each Sectional Council shall elect two Members to act on the General Council.

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year.

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34. Four Secretaries, one for each section, to be appointed by the Sectional Councils.

35. That, at all elections after the year 1882, each Fellow desirous of serving on a Sectional Council shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter his name on the list of candidates for the office of Councillor of a Section.

36. That all elections shall be by ballot.

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President the senior Member of Council then present shall preside.

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the senior Member of the Sectional Council present shall preside.

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previous to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

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Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members one fortnight previously, and the names of Candidates proposed for office must be in the hands of the Secretary one week before the meeting.

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may, and shall, on receiving a requisition signed by seven Fellows, at any time, on giving one week's notice, forthwith summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted.

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads:—Medical, Surgical, Pathological, and Obstetrical; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in regular rotation; the Council of the Pathological Section to have charge of the first meeting in each Session; that of the Surgical of the second; that of the Medical of the third; and that of the Obstetrical of the fourth; and so on until the end of the Session.[a]

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

Sub-Sections.

59. A Sub-section in Anatomy and Physiology, under the direction of the Surgical and Medical Sections, and a Sub-section in State Medicine, under the direction of the Medical Section, shall meet during the session, as may be arranged by the Councils of each Section.

[a] NOTE.—With the permission of the Colleges, the Sectional Meetings in Surgery and Pathology will be held in the Royal College of Surgeons; those in Medicine and Obstetrics in the King and Queen's College of Physicians.

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60. The Sub-sections shall have each a Chairman and Committee of six Members, of whom one shall act as Secretary.

Ordinary Meetings.—Order of Business.

61. (1.) Chair to be taken at 8.30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Obstetrical Section.

(5.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(6.) Chairman to call upon the author of the first paper on the list to read his paper.

(7.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(8.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(9.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

62. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.

(2.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(3.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(4.) Specimens must be in the room at 7.45 on the night of Exhibition.

(5.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(6.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

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(7.) The Exhibitor should be present, and he shall furnish further details if asked for.

(8.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

63. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman.

Bye-laws concerning "Transactions."

64. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

65. All Communications accepted by the Academy become the property of the Academy. Papers shall be handed to the Secretary of the Section immediately after they have been read.

66. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

67. The "Transactions" may be purchased by Members at cost price.

68. That the Publication Committee of each Section do meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals not later than Thursday afternoon.

69. That contributors of papers be requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof; that when one sheet of the "Transactions" has been completed it be printed off and laid aside until the termination of the session, and then bound up in the volume of "Transactions."

70. That on the evening of the day of meeting of the Sectional Council when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

(3.) That alterations be paid for by the author.

(4.) That illustrations be paid for by the author.

71. That an abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and that the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

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Expulsion of Fellow or Member.

72. Expulsion of a Fellow or Member can only take place at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

73. New Laws, or alterations in existing Laws, can only be proposed at the Annual General Meeting in October. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

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ANNUAL REPORT.

THE second year of the Academy of Medicine in Ireland has now closed, and the General Council are able to report that the success which attended the first Session has been fully maintained. It was believed by many that there would be a falling off in the number of Subscribers; but on the contrary there has been an increase under every denomination. The figures which follow show an advance:—

	1883–4.	1882–3.
Fellows	208	201
Members	27	21
Student Associates	24	22

The meetings were well attended throughout the Session, and the character of the work done may be judged from the volume of "Transactions" which has been published. The volume this year is 120 pages larger than that of 1883–4—a circumstance in part due to the fact that the Session began in November instead of December, and in part to the rule made in the Pathological Section, that no specimens should be there exhibited by card, but that all must be made the subject of oral communication.

The Council congratulate the Academy upon the very favourable reception accorded to the first volume of "Transactions" by the medical press in these countries and America, and they believe that the present volume will at least maintain the character of the first.

The Treasurer's Report shows that, including a balance of £228 19s. 6d., the receipts of the Academy for the year were £715 10s.; and the expenditure £410 7s. 3d., leaving a sum in hands of £305 2s 9d. to begin the Session.

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The King and Queen's College of Physicians and the Royal College of Surgeons continue to extend hospitality to the Academy by allowing the meetings of Councils, Sections, and Committees to take place as usual in their Halls, and the General Council desire again to express their warmest thanks for this kindness.

The question of paying for illustrations in the "Transactions" received very full and careful consideration, and, finally, on the motion of Dr. FINNY, seconded by Dr. BENNETT, the following resolution was passed:—

> "That the illustration of papers admitted into the 'Transactions' must be approved by the Council, and that at the pleasure of the Council a share not exceeding one half of the expense may be borne by the Academy."

The General Council have satisfaction in stating that a number of gentlemen residing outside Dublin have joined the Academy, and they believe that the wider interest that is being taken in its success will serve to make it a permanent Institution, and creditable to the Irish School of Medicine.

The General Council beg to submit the following resolution which was passed at the last meeting, on the motion of Dr. T. E. LITTLE, seconded by Dr. A. W. FOOT:—

> "That the Council desire to thank the General Secretary for his valuable services during the past Session, and recommend the presentation to him of £100."

By Order of the General Council,

W. THOMSON, M.A., F.R.C.S.,

General Secretary.

During the Session the following was forwarded to the General Council, and is now submitted to the General Meeting:

NEW RULE

Proposed by the Council of the Obstetrical Section—

> "That this Council recommends the abolition in their Section of the exhibition of specimens by cards."

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TREASURER'S REPORT.

RECEIPTS.

	£	s.	d.
To Balance from last year - - -	228	19	6
„ Fellows' Subscriptions - - -	447	6	0
„ Members' do. - - -	29	8	0
„ Student Associates' do. - - -	6	0	0
„ Sale of Tickets - - - -	3	16	6
Total Receipts - -	£715	10	0

EXPENDITURE.

	£	s.	d.
By Honorarium to General Secretary - -	100	0	0
Printing Circulars, Stamps, Post Cards, &c., &c. - - - - -	31	2	4
„ Reporters - - - - -	63	14	6
„ Servants, Porters, and Attendants at Colleges - - - - -	28	0	0
„ Messrs. Fannin & Co.— Printing "Transactions" and Postage -	132	1	9
„ Sundries, including Tea, Coffee, Milk, Cakes, Postage of Notices of Meeting, &c., &c. -	58	8	8
Total Expenditure - -	410	7	3
Balance in Bank - -	305	2	9
	£715	10	0

October 20th, 1884. Examined and found correct.

L. HEPENSTAL ORMSBY,
C. J. NIXON, } *Auditors.*

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TRANSACTIONS

OF THE

ACADEMY OF MEDICINE IN IRELAND.

MEDICAL SECTION.

ON LUPUS AND ITS TREATMENT.

By WALTER G. SMITH, M.D.;

Physician to Sir Patrick Dun's Hospital; King's Professor of Materia Medica, School of Physic, T.C.D.

[Read in the Medical Section, November 21, 1884.]

A CURSORY perusal of even a portion of the somewhat extensive literature which has grown up around lupus,[a] or a short experience of this affection, can scarcely lead to any other conclusion than that there are considerable difficulties besetting its study, especially in relation to the precise definition and limitations, the clinical relations, and the ætiology of lupus. Our ideas are, however, gradually assuming a more concrete form, and recent inquiries are calculated to place the matter upon a surer basis.

In the following communication I propose to make some remarks upon the nature and treatment of lupus, founded, in part, upon my own experience of the disease.

Let me first call attention to the summary statement of the cases

[a] The term lupus was first appropriated to the disease we now recognise as such by Sauvages, who called it "cancer lupus." The term "noli me tangere" is of more ancient date, and its origin is uncertain.

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of which I possess memoranda. A few recent cases are not embodied in the table:—

Year	Males	Females	Total
Hospital Cases—			
1869-72 - - -	3	8	11
1873-76 - - -	2	8	10
1877 - - - -	—	—	—
1878-81 - - -	13	22	35
Private Cases - - -	4	6	10
Total - -	22	44	66

It is plain that in drawing up statistics of an affection such as lupus, of debatable limits, the figures will be affected by the special views of the compiler—*i.e.*, whether he is inclined to assume a broad or narrow basis for diagnosis. Hence there is some uncertainty in comparing the numerical data given by observers in different countries. Taking, however, the above numbers, derived from an analysis of about 8,000 cases of cutaneous disease, I find that the frequency of occurrence of lupus in Ireland (Dublin) may be reckoned as approximately 1 in 200. In England and Scotland it seems to be much commoner. Dr. J. H. Stowers gives 2·5 per cent. as the result of his observations in London, and Dr. M'Call Anderson's statistics ascribe it a frequency of about 1 in 50.[a]

Professor Kaposi, of Vienna, has seen 1,200 cases of lupus in 20 years, while in the United States it is decidedly rare, and, according to the statistics of the American Dermatological Association, occurs in the ratio of only 1 in 400. Dr. Duhring remarks that the majority of cases appeared among the poorer Irish and German immigrants.

In my cases the ratio of females to males is 2 : 1, the preponderance in the female sex being in accord with the observation of most other writers.

In regard to the *differential diagnosis*, there are practically four

[a] Treatment of Diseases of the Skin, with an Analysis of 11,000 Consecutive Cases. 1872.

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other affections of the skin from which we have no discriminate lupus:—

1. *Scrofuloderma.*—This is confessedly sometimes a difficult question, and loses much of its significance if we reckon lupus as a tuberculosis. True primary lupus is rare in middle or advanced life; scrofulous disease of the skin is not so rare. Scrofuloderma sometimes responds very satisfactorily and rapidly to treatment, and ulcerations of years' standing may close up in a few weeks by the use of suitable measures.

2. *Syphilitic Lesions.*—While in the great majority of cases this is not difficult with sufficient care and experience, yet every now and then an example of ulcerating nodules about the nose, face, or scalp, raises the question, which is one of considerable practical moment. I have known a patch of tertiary syphilitic nodules "scraped" *secundum artem* in mistake for lupus. A course of iodide of potassium soon cleared away the nodules. Syphilis will destroy more in a few weeks than lupus in as many months or years, and the much greater rapidity of the destructive process in syphilitic lesions, coupled with the thicker crusting, are valuable elements in the diagnosis, which is clinched by the remarkable and gratifying cures for which we are indebted to the alkaline iodides.

And here I will enter a protest against a common and, in my opinion, mischievous error of nomenclature—viz., the term "syphilitic lupus."

There are few, I suppose, now who consider with Ricord that lupus is really allied to syphilis (a "scrofulate of syphilis"), or is an offshoot of hereditary syphilis (E. Wilson, Veiel); and whether we accept, or not, the doctrine of the specific tuberculous nature of lupus, we cannot at one time have a syphilitic lupus and at another time a non-syphilitic lupus.

It has been observed that when patients affected with lupus became syphilised, treatment by potassium iodide, while it removed the syphilitic lesions, left the lupus absolutely untouched.[a]

An occasional difficulty in diagnosis furnishes no reason for mixing up totally different affections. Lupus happening in a

[a] Edinburgh Med. Journ., Jan., 1884.

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syphilitic patient is not syphilitic lupus, just as lichen scrofulosorum is not an identical term with scrofulous lichen.

Hence, I think, the use of such loose terminology as syphilitic lupus, syphilitic psoriasis, &c., should be discountenanced. Let us rather speak of scaly, of tubercular syphilides, and so on. At the same time, I do not mean to deny the modifying influence of diathesis upon local lesions, nor should its consideration be neglected.

The term "lupoid," so often heard, is likewise an objectionable one, sometimes surely nought but a thin cloak for ignorance, and dates from a time when less care perhaps was taken, or opportunity existed, for framing an exact diagnosis. If we do not know, or cannot formulate a diagnosis in a given case, let it pass; but why say "lupoid" when we do not say "syphiloid?" It is time for *lupoid* to slide out of use, as has happened, *e.g.*, with the word *cancroid*.

3. *Psoriasis.*—Mr. Jonathan Hutchinson's proposal to designate certain obstinate scaly red patches by the hybrid term psoriasis-lupus is, I think, a step rather in the direction of confusion than of simplification of our ideas. Still it is curious to note that occasionally we do meet with cases wherein a doubt may arise as to the diagnosis between lupus and psoriasis, and in which an attentive consideration of the history and character of the affection is requisite in order to form a correct opinion.[a]

4. It may seem strange to allude to the possibility of mistaking lupus for chronic eczema, or *vice versâ*. But, independently of the fact that lupous surfaces are liable to attacks of intercurrent superficial inflammation which may mask the lupus, I have seen a few cases of superficial lupus confined solely to the helices of the ears, where the superficial appearances almost exactly simulated those of eczema. The diagnosis rests upon the extreme chronicity of the process, and especially upon the existence of scarring, and other evidences of friability and loss of tissue. Primary lupus of the inside of the nostrils may, too, easily escape detection, and it is probable that some cases of supposed intractible "eczema" of

[a] *Cf.* Plates XIX., XXXVII. New Syd. Soc. Atlas.

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the nasal mucous membrane are really lupus. Lupous perforation of the cartilaginous septum may occur without any external manifestation of the disease, but I have never known the vomer to be attacked. This leads me to refer to lupus on mucous membranes elsewhere. In cases of long-established facial lupus it is not uncommon to meet with lupus on the gums, showing itself as a well-defined granular red fringe. In all the cases I have observed the disease commenced in, and was usually limited to, the region of the incisor teeth of the upper jaw.

Nor is lupus very rare upon the palate, but I have seen only one case of lupus invading the larynx. Lupus of the conjunctiva is common enough from extension of facial lupus, but primary lupus of the conjunctiva is rare, and I have seen but a few cases where this diagnosis was made.

Of lupus involving the vulva I have met with only one case.[a]

Among the rarer remote consequence of old-standing lupous disease is deformity of the parts (*lupus mutilans*), which, when it occurs on the hands, as I have seen, bears no small resemblance to leprous deformity.[b]

In one case I have met with the unfortunate event of the supervention of malignant disease upon lupus. A man, aged thirty-five years, who consulted me for extensive lupus of the face and nose, presented himself subsequently with a large, dark, fungating malignant growth springing from the nose. This growth was removed by my colleague, Dr. C. Ball, but the patient succumbed to a recurrence of the malignant growths in the course of the year.[c]

Turning now to the pathology of lupus, we find two chief views prevailing as to its ætiology—

1. What may be termed the Anglo-French school—*i.e.*, that lupus has a constitutional foundation, and is allied especially to scrofula.

[a] *Cf.* Dr. Matthews Duncan. Edinb. Med. Journ. July, 1884. Med. Times and Gaz. November 15, 1884. Dr. A. Macdonald. Edinb. Med. Journ. April, 1884.

[b] *Cf.* Drawing in Virchow's Archiv. 1883. P. 218.

[c] "Auf Lupus eine sehr deletäre Form von Carcinom entstehen kann" (Kaposi).

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Mr. Jonathan Hutchinson[a] seeks, with Auspitz, to widen the signification of lupus, and contends for a clinical "lupus family" of affections. He regards lupus on the whole "as a sort of cross produced by tendencies at once to scrofula and cancer, while it receives many modifications, from peculiarities in the patient's skin and his morbid tendencies, in one or the other direction." It is interesting to note that a similar view was put forward as novel at the time by Dr. James Houghton, fifty years ago:—"Were we called on to declare our opinion of the essential character of lupus, we should say that it is an intermediate pathological state between cancer and scrofula, partaking somewhat of the nature of both, but constituting a state in which, by the blending of these two diseases, many of their peculiar characteristics are lost."[b]

2. The Vienna school, as represented by Kaposi—that lupus is an exclusively local affection, and hence constitutional treatment is rejected as useless. But, notwithstanding Kaposi and the arguments he adduces, converging evidence has been accumulating in favour of the doctrine that lupus is a branch of the tuberculous stock; or, in other words, that lupus will find its true place among the chronic infective diseases of the skin—*i.e.*, those dependent upon the action of an organised virus capable of reproducing itself in the body (*e.g.*, lepra, syphilis, and tuberculosis).

The following remarks refer particularly to lupus vulgaris, for the nature of lupus erythematosus is still a matter of controversy, and Veiel[c] places it among the superficial inflammations of the skin along with eczema and impetigo.

The question, then, is this—Syphilis has a specific virus, likewise leprosy, and tuberculosis; is it so with lupus?

Ziegler,[d] while he places lupus among the infective granulomata, admits that "the exciting cause of lupus is unknown." Similarly Hyde[e]—"The causes of lupus vulgaris are absolutely unknown;"

[a] Pedigree of Disease. 1884.
[b] Cyclop. Pract. Med. III. 1834. Art., Noli Me Tangere. P. 177.
[c] Ziemssen's Handbuch der Hautkrankheiten. 1883.
[d] Patholog. Anatomy. Macalister. Part. I. 1883.
[e] Diseases of the Skin. 1883.

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and Neisser,[a] in his admirable article, to which I am largely indebted, and in which he upholds on general pathological grounds the tuberculous nature of lupus, says:—"But I cannot yet adduce the exact proof of this connexion, since neither I nor others have hitherto succeeded in demonstrating with certainty the bacilli of tuberculosis in lupous material." He presently adds that he holds the forthcoming proof to be only a question of time.

The question may be conveniently studied from three points of view—viz., clinical, histological, and experimental.

1. *Clinical Aspects.*—Upon this point suffice it to say that while Kaposi and his followers are unable to see any connexion with scrofula or tubercle, and even ridicule the proposition, they have arrayed against them the testimony of numerous skilled observers in England, France, and Germany, who recognise the points of resemblance, and note the frequent coincidence of cheesy affections of the glands, bones, and joints with lupus.[b] I cannot give statistics from my own cases, but certainly the association of lupus with scrofulous glands is sufficiently common here, and, with Fagge and others, I have witnessed the development of lupus secondary to suppurative strumous inflammation.

Now, since the fundamental unity of scrofulosis and tuberculosis has been established both on clinical and experimental grounds, if it can be shown that an intimate relation exists between lupus and scrofula an argument will be furnished for bringing lupus into the tuberculous family. M. Besnier, one of the foremost French dermatologists, insists upon the connexion between lupus and tuberculosis. In June and July, 1883, among 38 patients under his care for lupus in St. Louis, 8 presented well-marked physical signs of phthisis.[c] Dr. Tilbury Fox[d] states that lupus, in many cases, occurs in phthisical subjects; and Mr. Hutchinson has pointed out that phthisis is not infrequently observed in the families of those suffering from lupus.

[a] Ziemssen. Handbuch der Hautkrankheiten.

[b] *Cf.* Discussion on Lupus, Section of Dermatology and Syphilis. Internat. Med. Congress, Copenhagen. 1884.

[c] London Med. Rec. March, 1884.

[d] Skin Diseases. Third Edition. 1873.

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It is quite true that lupus is rarely observed in several members of the same family (Fagge), that it is seldom found in combination with general tuberculosis, and that we have no evidence of hereditary or of direct transmission in the human subject.

2. *Histological.* — Careful investigations have shown that no essential difference can be established between a caseating miliary tubercle and a lupous nodule, which sometimes exhibits "the exact appearance of tubercles" (Ziegler). The pathological processes in each are the same in kind, but differ in degree. Thus, in lupus it is less acute and less intense, and hence we get slower development of the inflammatory granuloma with a richer development of vessels, and consequently a more gradual destruction towards the centre, with peripheral healing and formation of spindle-cell tissue—*i.e.*, cicatrix (Neisser). But the decisive proof—the demonstration of tubercle-bacilli in lupus-material—remained to be given, and it was not long before Neisser's prophecy was fulfilled. Dr. Robert Koch, following up Friedländer's anatomical investigations, examined 7 cases of lupus of unimpeachable diagnosis.

In 4 cases he excised parts of the skin. In 3 cases he examined scrapings only of the lupus-tissue.

For direct microscopic investigation he used only the excised bits of skin. The tubercle-bacilli were found sparsely in each of the 4 cases, and only in the interior of the giant-cells. The tubercle-bacilli in lupous tissue are so isolated that in 2 cases the bacilli were not found until in the one case 27 sections and in the other 43 sections had been made. Yet it repeatedly happened that when in a number of sections not a single bacillus appeared, sections taken close by exhibited 1 to 3 bacilli. Koch never found more than one bacillus in a giant-cell.[a]

According to Unna, the bacilli are observable in quantity by partially digesting hardened specimens and examining the precipitate that falls down. Demme, Pfeiffer, and Doutrelepont had published records of the occurrence of tubercle-bacilli in lupous skin, and in the tubercles of animals inoculated with lupus. But

[a] *Cf.* Figs. 29, 30, Mittheilungen aus dem kaiserlichen Gesundheitsamte, 1884.

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Koch states that his experiments were finished for some months before their communications were published.[a]

The curiously sparse occurrence of the bacilli in lupus suggests a ready explanation of some of the negative results of other histologists, and likewise forbids the hope of deriving material help in diagnosis from the use of the microscope. So far as I am aware, bacilli have not been demonstrated in connexion with lupus erythematosus.

3. *Experimental.*—Koch[b] made inoculations from all his seven cases into the anterior chamber of the eye of rabbits. In every case this was followed by tuberculosis of the iris, and in those animals which lived long enough by general tuberculosis. Numerous tubercle-bacilli were found in these inoculation-tubercles. From one specimen (excised from the cheek of a boy ten years old) he obtained pure cultures, which were several times utilised for successful inoculations on animals.

Again, Pagenstecher made three inoculations from conjunctival lupus into the anterior chamber of the eyes of rabbits. In two cases he succeeded, in one he failed. Microscopical examination of the two successful cases by Pfeiffer (Ehrlich's method) exhibited Koch's bacilli, duly recognised as such by Ziegler, Ehrlich, and others.[c] Positive results such as these, coupled with those of Schüller and Hüter, more than counterbalance the negative results announced by Cohnheim, Hänsell, and others.

Two years ago Vidal and Leloir could assert that "no results have been obtained from the experimental inoculation of animals with lupus."[d] Gathering together, then, the foregoing evidence, we seem to be guided to the conclusion that lupus is a tuberculosis (scrofulosis) of the skin excited by the tubercle-bacillus. The localisation of the bacillus in the skin, and the relatively rare involvement of other organs, constitute the peculiar features of

[a] *Cf.* Morison on Lupus and Tuberculosis. Amer. Journ. Med. Sci. April, 1884.

[b] Loc. cit.

[c] Berl. klin. Wochenschrift. 7 Mai, 1883.

[d] Journ. of Cutan. and Vener. Dis. 1882. P. 349.

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lupus as compared with other forms of tuberculosis. The bacilli of lupus and tubercle are probably the same qualitatively, but there is a quantitative difference which is accentuated by the more unfavourable conditions of nutrition in the colder skin. Complications with tuberculous affections of other organs—*e.g.*, glands, joints, bones, and even with analogous skin affections—*e.g.*, ulcerating scrofulides, are frequent. Their non-occurrence does not contradict the tuberculous ætiology of lupus.

Genetically, then, there is only one tuberculosis of the skin, and we may say that while lupus is always tuberculosis of the skin, yet tuberculosis of the skin assumes other forms than that of lupus (Neisser). The subjoined table may be useful for reference :—

TUBERCULOSIS OF THE SKIN.

1. *Miliary Tuberculosis.*	2. *Lupus* (*Tuberculo-derma*).	3. *Ulcerating Scrofulo-derma.*
Miliary tubercles and ulcers as part of a general tuberculosis. Caseates. Very rare.	A harmless (*quoad* system) form of tuberculosis, mostly limited to the skin. Does not caseate.	*i.e.*, ulcerations arising from bursting of a "cold" subcutaneous abscess. Often yields well to treatment.

Symmetrical.	*Unsymmetrical.*
Lupus erythematosus. L. sebaceus, &c. Very difficult to cure.	Lupus vulgaris. L. exulcerans, &c. Local malignancy.

Aberrant forms.

In short, the evidence is strong of the unity of cause in tuberculosis, lupus, and scrofulosis, although we do not yet know the special determining conditions of each case. Ewald[a] even goes so far as to suggest *Morbus Kochii* as a clinical term for this group of three, by way of analogy to Morbus Brightii, but this innovation is scarcely likely to be approved.

If asked for a definition of lupus, I would say it is "a very chronic new-cell growth, depending upon infection with the bacillus

[a] Berl. klin. Wochenschrift. No. 44. 1884.

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tuberculosis, always ending by scars, with or without ulceration, and usually developing brownish-red nodules."

Treatment of Lupus.—The two prime objects of treatment are—(1) to check the development and extension of the morbific virus; (2) to destroy the existing foci of disease. Constitutional treatment can bear upon the first object only; local treatment has a relation to both objects. In regard to constitutional treatment, I will only say that, if the tuberculous doctrine of lupus be accepted, it lends emphasis to the recommendations of those who advocate the internal use of anti-scrofulous remedies, and especially cod-liver oil, iodine, iodoform, and the adoption of all measures calculated to fortify the system, while, on the other hand, it adds force to those who teach that enfeebling remedies, such as mercury, should be abstained from.

I may also point out that some of those who refuse to acknowledge a relationship between lupus and scrofula, nevertheless appreciate the use of such therapeutic means as those just referred to, which may be supposed to act by modifying the soil, for "we become tuberculous, we are born scrofulous" (Landouzy). Dr. J. Warburton Begbie[a] mentions that, many years ago, when a student in Paris, he saw numerous cases of ulcerating lupus in the wards of St. Louis Hospital, which were materially benefited, and some apparently cured, by the administration of cod-liver oil in very large doses. The patients took the oil, not in spoonful doses, but in *large glasses or tumblerfuls.*

The late Dr. Edmund Parkes, who completed and edited Dr. A. Todd Thomson's "Treatise on Diseases of the Skin, 1850," testifies that Dr. Thomson was extremely successful in temporarily, and sometimes even permanently, arresting the ravages of lupus. Dr. Parkes saw many cases treated by him in which "a speedy, manifest, and undoubted improvement followed the use of certain remedies," chiefly iodine, iron, and arsenic conjointly, while local applications were sparingly used. Cod-liver oil was frequently given.

On the whole, in reference to the internal medication of lupus, we cannot maintain that as yet we know of any specific against

[a] Works, New Syd. Soc. 1882. P. 319.

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the lupus (*i.e.*, tuberculous) virus; and perhaps the most that can be done in this way is by strengthening the constitution to increase the capacity for resistance of the body against the spread of the germs of the disease.

It will be generally admitted that local treatment is the more important, and the principles to bear in mind are these:—

(*a*) To remove useless and morbid tissue.

(*b*) To effect in those parts which are still firm and comparatively healthy the absorption of the lupous infiltration.

(*c*) To vigorously suppress any relapse at once (Volkmann).

Locally, in spite of the easy accessibility of the lupus foci, our therapeutical efforts are not always rewarded by success. As to erythematous lupus, let us hear what MM. Besnier and Doyon say:—"Nothing is more deceptive than the therapeutics of lupus erythematosus, even allowing for recent incontestable advances. Spontaneous cures, speedy success with the most simple and the most diverse methods, frequent relapses, often unsuccess even when recourse is had to the most active measures—this is what the practitioner has to expect in the treatment of lupus erythematosus."

Before entering upon the discussion of the choice of special remedies, the following propositions may be laid down:—

(*a*) There is no single lupus panacea to be looked for.

(*b*) No individual method will suit every case.

(*c*) Relapses will occur in a considerable number of cases, spite of any and every treatment.

(*d*) When there is much irritation, soothing treatment should be first adopted. Severe local procedures sometimes do more harm than good.

For the local extirpation of the new growth the chief methods have been:—

1. Chemical agents:

(*α*) *Elements—e.g.*, iodine.

(*β*) *Acids—e.g.*, arsenious, salicylic (carbolic and pyrogallic).

(*γ*) *Bases—e.g.*, the caustic alkalies, including Vienna paste and London paste.

(*δ*) *Salts*, of silver, mercury, and zinc (Landolfi's paste).

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From a practical point of view, chemical caustics may be divided into those that destroy indiscriminately both sound and diseased tissues—*e.g.*, caustic potash, Vienna paste, Landolfi's paste; and those that, when carefully used, destroy only the diseased tissue—*e.g.*, arsenious acid and pyrogallic acid.

Neisser[a] strongly recommends pyrogallic acid, as introduced by Jarisch, and, from my own limited experience of it, I am inclined to endorse their recommendation.

Boring with a pointed stick of lunar caustic combines the effects of mechanical destruction with a corrosive action, but is very painful.

2. Thermal—*e.g.*, the actual cautery, or the galvanic cautery, or Paquelin's instrument.

3. Mechanical measures—*e.g.*, excision.

Within the last few years much attention has been directed to certain modifications of operative treatment. Of these may be named:—

(*a*) Multiple puncture, introduced by Veiel (1871) and Dubini, and advocated by Volkmann.

This method is wanting in precision, and is applicable to only a few cases of limited extent, and in an early stage.

(*b*) Linear scarification, an extension of the idea of multiple puncture. It was introduced by Mr. Balmanno Squire in 1880,[b] and is largely used in France by Vidal and Besnier.

The object of each of these methods is to starve the neoplasm by cutting off or interfering with its blood-supply.

(*c*) The curette, or sharp spoon (Volkmann, 1870), or method of erasion.[c]

This method of treatment is best followed up by immediate cauterisation of the raw surface, in the hope of destroying the outlying germs of the disease.

I have had some experience of these methods, each of which,

[a] Loc. cit.

[b] Brit. Med. Journ. May 1st, 1880.

[c] *Cf.* Malcolm Morris on the Comparative Advantages of Scraping and Scarification in the Treatment of Lupus Vulgaris. Brit. Med. Journ., Aug. 18, 1883.

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perhaps, has its own proper sphere, but, speaking broadly, I prefer the method by scraping, or erasion. And a comparison of the practice and results of the treatment of lupus at the present time with the results expected or attained twenty or even ten years go will show, beyond question, that if lupus cannot as yet be expunged from the list of *opprobria medicinæ*, still the disease has been deprived of some of its repulsiveness, its ravages have been more effectually checked, and its prognosis has been materially brightened.

I shall not now enter into details of the actual procedure in either case, but will content myself, in conclusion, with summarily specifying the individual features of these two methods.

Erasion, or scraping, then—

1. Differentiates sound from diseased tissue, for healthy skin will not give way to the spoon.

2. It is rapid of execution.

3. It is, as a rule, not followed by much after-pain.

4. The scraped surface heals wonderfully quickly.

5. It leaves a level and tolerably sightly cicatrix.

Scarification—

1. Is applicable to some situations, and, in some cases, where erasion is unsuitable or inadvisable.

2. It is less painful, and, to some persons, a less repellant operation.

3. There is a minimal loss of substance.

4. It is especially adapted for diffuse non-ulcerating infiltrations.

5. The scar left is scarcely distinguishable from the healthy skin, and it is said not to be liable to cheloid growths, which sometimes develop upon the scars left by scraping operations.

Hence, in lupus of the face, where æsthetic considerations have especial force, linear scarification deserves a full trial in the promise it affords of effecting the desired object with the least amount of consequent deformity.

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CLINICAL HISTORY OF A CASE OF ANURIA, WITH A DESCRIPTION OF THE MORBID APPEARANCES AND OF THE PATHOLOGICAL HISTOLOGY.

By WALTER BERNARD, F.K.Q.C.P.I.,
Londonderry.

[Read in the Medical Section, November 21, 1884.]

I HAVE thought the following case worthy of bringing under your notice, because it has many interesting features in itself, as will appear in the sequel, and because I have observed the case almost daily and sometimes three times a day during life, and have been able to procure the diseased tissues, and to get excellent microscopical sections. It is rather a rare event in a small provincial town to be able to carry out *post mortems* to one's satisfaction. I have to thank a very able anatomist and pathologist—Mr. Shattock, of University College, London—for his very kind and patient investigation of the diseased tissues, and for much information bearing on the case :—

Clinical History.—In December, 1883, A. M., aged seventy-five years, a healthy-looking old man of active habits, sent for me. He complained of having to urinate much more frequently than formerly. I had some difficulty in persuading him to use a soft catheter. In a short time he improved and discontinued the use of the catheter.

In February, 1884, I was again sent for, and found he had been bleeding from the nose all night. The usual styptics failed to arrest the hæmorrhage, and I was obliged to plug. A short time afterwards severe hæmaturia set in. A drop of the urine put under the microscope showed abundant red cells. The guaiacum and turpentine test was also used. The hæmaturia was accom-

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panied by rigors, diarrhœa, and thirst. Temperature rose a little above normal. At this time the urine measured 84 ozs. in twenty-four hours, was acid, with a sp. gr. of 1010. Patient complained of no pain. No tumour could be felt in either lumbar region. Pulse was intermittent, and averaged 48 in the minute. This attack of hæmaturia gradually passed off, but left him in a wasted and anæmic condition. He slowly recovered, and continued to use a catheter. On the 22nd of May I again saw him. He was then vomiting a green fluid; this lasted two days. The pulse, which had become normal after the hæmaturia ceased, again began to intermit, and again fell to 48 in the minute. He complained of uneasiness over the right lumbar region, but no tumour could be felt. When vomiting ceased he suffered from thirst. His urine averaged 84 ozs. in twenty-four hours. Prostate at this time felt hard and large, especially its left lobe. On May 28th urine was again bloody—sp. gr. 1010; normal quantity passed. Again vomiting of green matter set in with the slow and intermittent pulse. During this attack, which lasted two days, he felt drowsy.

From the 6th to the 17th of June there was complete suppression of urine. During all this time he was free from pain, and seemed to suffer no inconvenience. I believe there is no doubt about the suppression. A catheter was frequently passed into the bladder in the presence of other medical men. Palpation and percussion revealed no tumour above the pubes. He was most carefully watched day and night to see if any urine escaped. Not the slightest indication of moisture could be detected on the sheets or on his linen. While the suppression lasted there was no desire whatsoever to urinate, and he felt so well that he wanted to get out of bed and go about his business. His appetite was good, and he slept well; his bowels moved once daily; there was less drowsiness than before the suppression. A hypodermic of pilocarpine salivated him, and caused him to perspire profusely, but had no good effect. Digitalis stupes were applied on the 16th of June—eleventh day. On the following day urine commenced to flow, and averaged 140 ozs. to 125 ozs. in twenty-four hours to the 21st of June. On the 22nd of June 192 ozs. of bloody urine was passed. When urine was clear it was tested for sugar and albumen, but neither was detected; but one constant character in the flow all through was, whether it was bloody or clear, that it always showed to different urinometers a sp. gr. of 1010, and was always acid.

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23rd of June.—There was wasting, hoarseness, frequent pulse, subnormal temperature; eyes had a sunken appearance; tongue was dry; urine was smoky, amounting to 80 ozs. in twenty-four hours; superficial veins on abdomen and backs of hands large; marked aortic pulsation to the left of epigastrium. Next day (24th June) urine clearing, 68 ozs. in quantity; voice stronger; looks better; tongue moist; sat up in bed. Pulse regular, 60.

25th.—Superficial veins and pulsation gone.

26th.—He was so well that he insisted on getting out of bed and going out of doors; 80 ozs. of clear, pale urine passed; is thirsty; drinks koumiss.

27th.—80 ozs. of bloody urine passed; feels well, and does not complain.

28th.—82 ozs. of bloody urine passed; still feels well.

July 2nd.—80 ozs. with very little blood.

3rd.—Urine free from blood, pale. Patient got out of bed. When he moves about now he has to make his water frequently. Is able to empty his bladder fairly well; a few teaspoonfuls of urine were removed by the catheter after micturition. Feels drowsy.

4th and 5th.—Suppression of urine; no thirst; drowsiness gone; complains of nothing save an uneasiness over the right kidney.

6th, third day.—Passed 17 ozs. mixed with blood during the twenty-four hours.

8th.—Passed 13 ozs. of bloody urine.

During the following week nothing worthy of note occurred, except the suppression of urine for one day.

16th.—He began to lose power over the neck of his bladder during sleep. Urine varies—sometimes it is quite free from blood, at other times is well coloured. Patient takes a walk in the fields. He complains of a frequent desire to micturate, and of hoarseness and sore throat. The pharynx and tonsils are red and slightly swollen.

From the 16th of July to the 10th of August nothing very remarkable occurred. During that time there was no blood in the urine, which was pale and of the same sp. gr., 1010. The desire to micturate became more frequent. Neither use of catheter nor rest in bed relieved it. On July 27th a large lumbricus, eight inches long, crawled up his throat, and was discharged by the mouth.

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August 10th.—I felt a small tumour above the pubes, movable from side to side, hard but elastic; could cause it to move between my finger in the rectum and my hand above the pubes. Catheter only brought away a spoonful of water. The desire to pass water was now almost constant. There was no difficulty in keeping him in bed.

17th.—Appetite good; no decided uræmic odour from skin; no drowsiness; wasting marked.

31st.—Tumour larger than a cricket ball. When I press on tumour above pubes he feels a strong desire to micturate; gets out of bed, without showing any signs of weakness, to make water almost every half hour; refuses to use a urinal; retches occasionally, but does not vomit his food.

Sept. 7th.—There is now a strong odour from the skin; retching is frequent; drowsiness almost constant. Pulse, 84 to 96; temperature, 97·2°. Appetite and strength failing; wasting very rapid; tumour increasing upwards, backwards, and towards right side, and becoming harder. There is loss of voice.

13th.—Patient died.

Eleven and a half hours after death, with the assistance of Dr. Craig, I removed the urethra, bladder, ureters, kidneys, and a small portion of the liver, and got Mr. Shattock to examine them for me. I will now read his report and exhibit specimens:—

Report by S. G. Shattock, F.R.C.S., London.

The material consists of the bladder and kidneys, with a small portion of the liver, all preserved in spirit.

The bladder contained about an ounce and a half of pale yellow turbid fluid. Its muscular wall is slightly hypertrophied, its interior fascicular.

The mucous membrane of the bladder is extensively destroyed by ulceration, in parts so deeply as to expose the muscular coat. Over the upper part of the bladder, however, the mucous membrane is entire, and is here raised in numerous small hemispheroid or lenticular swellings. The largest of them is 7 millimetres in its chief diameter. The rest are considerably smaller. The mucous membrane admits of ready removal from the subjacent muscular coat, the dissection showing the eminences to be due to distinct neoplasms, which involve the membrane. On sections the neoplasms are fairly firm and milky white. Towards the general ulcerated area, the summits of the neoplasms are ulcerated,

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and at the margin the general ulcer has evidently resulted from the coalescence of many smaller ulcers which have spread from similar points of origin.

Superficial ulceration extends from the bladder into the prostatic portion of the urethra, but beyond this the urethral mucous membrane is healthy.

The tissues lying round the lower part of the bladder are thickened and indurated; around the base and the vesiculæ seminales the connective tissue is softened and shreddy. The peritoneum is inseparable from the bladder, except over its summit.

On slicing the prostate, it is seen to be infiltrated with a soft white growth which, for the most part, has replaced the proper glandular structure. The new growth spreads through the gland in variously branching lobular masses, and has not materially altered its figure, except in producing some enlargement of the left lateral lobe.

The vesiculæ and vasa deferentia appear normal. Both the ureters are equally and considerably dilated, tortuous, and ampullary, the enlargement involving also the pelvis and calyces of the kidneys.

Kidneys.—These are of the normal size—the right measures 4⅜ inches in length; the surface is smooth, and the capsule readily separable. On section they are pale, firm, resilient, and a lens discloses large numbers of minute cysts scattered throughout the cortex. On pouring iodine solution over washed slices the parts stain uniformly brown, and present nothing characteristic of amyloid degeneration.

In the hilum the connective tissue is unnaturally dense, the vessels and renal pelvis being firmly matted together. The renal papillæ present the early flattened condition associated with chronic distension of the pelvis and ureter.

Liver.—The portion furnished cuts firmly and uniformly, and the lobules are without difficulty distinguishable. The central parts of each are stained more yellow than the rest. On pouring iodine solution over the washed surface the tissue stains uniformly, without giving evidence of amyloid change. In some sections are displayed, sparsely scattered, well-defined circular white areas; the largest measures 3 millimetres in diameter, others are of the size of pins' heads.

Morbid Histology.—Parts of the liver and kidneys were hardened in strong spirit, afterwards soaked in water, and then in a mixture

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of mucilage 2 parts, syrup 1 part, and afterwards frozen, and cut on a Swift's microtome. The sections were washed in distilled water, and stained with logwood, again washed, the water extracted with absolute alcohol, and then rendered transparent with clove oil; they were then finally mounted in Canada balsam dissolved in chloroform.

Histological Report.

Kidneys.—Sections of the cortex display a perfectly normal condition of the Malpighian corpuscles and renal tubuli, but here and there spaces occur of various form, according to the mode of section—circular or elongated and widening; these are of larger diameter than the normal tubuli, and are devoid of epithelium or contents in the sections. The amount of intertubular stroma is not recognisably increased.

In one kidney the apices of some of the pyramids present a coarser cystic honeycomb-like condition.

Except in these parts the histology of the pyramids is normal, though similar cystic dilatations of tubuli are common, as in the cortex.

Liver.—The histology is normal, except for the neoplasms before referred to.

The new growths have a gland-like construction, and consist of scanty connective tissue forming alveoli, which hold multiform epithelial cells.

In some sections the more central epithelial cells of the alveoli have fallen out, so as to produce a finely perforated or sieve-like appearance, discernible with the naked eye.

Bladder.—The growths affecting the mucous membrane resemble those described in the liver. Widespread extensions of similar morbid growth traverse the interfascicular tissue of the muscular coat, the epithelial character of the cells being in these places quite distinct, and in a better state of preservation than on those portions of the growth which have been for some time macerating after death in the fluid contained in the bladder.

Prostate.—The growth consists of multiform epithelial cells closely pressed together in a lobulated scanty fibrous stroma. The elements are so densely packed that it is only in places that the stroma is satisfactorily discernible.

Remarks.—The hypertrophy and slight sacculation of the bladder, with the dilatation of the ureters, testify to an obstruction to the

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exit of urine, of slow onset, but which has not progressed to any serious degree. The dilated tubuli in the renal substance are to be ascribed to this chronic distension of the ureters.

There is nothing in the morbid histology to explain, *per se*, the suppressed secretion of urine.

The obstruction to the exit of urine from the bladder is to be ascribed to the morbid growth infiltrating the prostate, and to the growths which have involved the parts about the neck of the bladder.

The new growths within the liver are disseminated carcinomata, produced by secondary infection from the primary growth within the prostate; and the multiple tumours projecting into the bladder must also be explained as due to infection from the original growth within the prostate, like the widely strewn cutaneous nodules sometimes occurring around a carcinoma originating in the mamma.

The primary growth is evidently that affecting the prostate, since this is by far the most extensive, and it is only in the prostate that a gland tissue exists to furnish the histological basis of the morbid growth.

Lastly, as regards the ulceration of the bladder, it may be assumed, as told from the spreading margin before described, that it has resulted from the breaking down of closely set tumours, though the other more common conditions of inflammation, &c., may have exaggerated its degree.

Remarks on the whole Case.

Cases of carcinoma of the prostate are not very common. Scirrhus appears to be very rare, but softer forms of carcinoma occasionally attack the prostate. In this case the bladder and liver were also involved.

The most important symptom in the whole case is the suppression of urine, which occurred on three occasions. On the first occasion it lasted eleven days, on the second it lasted three days, and on the third one day. Now, it is very difficult to explain this symptom. Mr. Shattock, in his Report, says—"There is nothing in the morbid histology to explain, *per se*, the suppressed secretion of urine." Sir Henry Thompson, with his usual kindness, replied to a letter of mine, and suggested that the suppression was probably

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for the most part *reflex*. This is, I think, the best explanation that can be given.

There is another point well worthy of notice. The ureters and kidneys show signs of obstruction much more than the bladder.

Mr. Shattock, in a letter which followed the Report, says the hypertrophy of the bladder is slight—in fact, he says, it may remain an open question whether the bladder is hypertrophied or not. In cases of prolonged obstruction in the urethra or neck of the bladder, the bladder becomes hypertrophied, and by reason of its increased thickness prevents the urine from entering the bladder from the ureters. Now, in this case there was not enough hypertrophy to account for the condition of the ureters and kidneys. The morbid growths caused obstruction round the orifices of the ureters, and brought about the condition found *post mortem*.

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HEREDITARY AMAUROSIS.

By JOHN B. STORY, M.B., F.R.C.S.;
Surgeon to St. Mark's Ophthalmic Hospital, Dublin.

[Read in the Medical Section, December 19, 1884.]

THE affection which forms the subject of this paper is very incompletely described by the term "hereditary amaurosis"—a title which was given to it by S. Sedgwick in *The Medical Times and Gazette* in 1862. The disease is more correctly defined by Leber in the article on "Diseases of the Retina and Optic Nerve," in the Graefe and Sœmisch Handbook, as *optic neuritis resulting from hereditary and congenital predisposition.* That description of Leber's is based upon the records of fifty-five cases occurring in sixteen families, and so far as I can ascertain there has been hardly any important addition to the literature of this exceedingly interesting affection since Leber's article, published in 1877.

The disease generally appears some years after the age of puberty. In ten of the sixteen families it occurred in only one generation; in the other six, two generations were affected; it has never been observed as yet in three successive generations. In no case was the father affected, and the mother was affected in only two instances, both of them being, according to Leber, rather unusual forms of the affection. In the one case the disease appeared at the climacteric period (as it did in my first case), and in the other bad sight was stated to have been present from the fifth year of life. Females are generally spared (only ten per cent. of the patients were females), but in five of the sixteen families the disease was inherited through the mother, though she herself was unaffected. In but one family did consanguinity play any part in the production of the disease, and in that the consanguinity occurred in the great grandparents.

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The patients belong to the neuropathic class of Griesinger; many of them suffer from headache, migraine, vertigo, palpitation, tingling of the limbs, and convulsive attacks; they are excitable and nervous, and one is stated to have had epileptic seizures.

The period of life at which the attack commonly commences varies between the 18th and 23rd year, but it has been observed from the 5th and 13th up to the 28th and 43rd years.

Both eyes have always been attacked, mostly with a short interval, six months being the longest interval observed by Leber.

The defect of sight generally makes its first appearance suddenly as a cloud or haze, which mostly increases for the next fortnight or month. Pain in the eyes or head is exceptional; subjective light and colour sensations are occasionally observed. The defect is mostly a pure central amblyopia often appearing as a distinct central scotoma, while the peripheral limits of the fields of vision are little, if at all, contracted. Hemeralopia, by which is meant that the patient sees relatively better in twilight, as is usual in retrobulbar neuritis, is a constant symptom.

Ophthalmoscopically there is usually found slight haziness of the disc margins, and hyperæmia of the papilla, the arteries not being contracted, but sometimes rather dilated, and white lines may be seen running along the edges of the vessels. Sometimes a peculiar streaky exudation is observed, and sometimes the appearances, in spite of advanced blindness, are altogether normal. After a time white atrophy of the papilla is observed, the nasal half being occasionally less affected than the temporal, and the retinal vessels are moderately contracted.

Recovery of sight is very rare, but there is usually sufficient peripheral vision left to enable the patient to walk about freely alone. Absolute blindness is, fortunately, also uncommon.

Treatment is absolutely unsuccessful. Mercury, iodide of potash, diaphoresis, local blood letting, nitrate of silver, strychnine, and galvanism of the sympathetic, have all been tried without distinct benefit.

Prof. Fuchs has recorded twelve cases of the kind in the *Klin. Monats. Bl. f. Augenheilk.*, for 1879, p. 332. Unfortunately the

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large majority of them were known to him only through description by relatives, but there can be little doubt that the diseased condition was essentially the same as in the cases described by Leber. All Fuch's cases were males. The first family consisted of four daughters and four sons; of these, three (males) became affected. The second family consisted of four daughters and two sons; both the sons were affected. One of the healthy daughters married and had five sons, all of whom were attacked; she also had one daughter, who escaped. The third family contained two sons, both of whom were affected, each having the central scotomata which characterise the disease.

To the already published cases I can now add five, all occurring in one family, but only two of the five have been actually under my own observation. One of these patients was a female, and this case exhibited a symmetrical defect in the fields of vision.

Case I.—Miss S., aged forty, consulted me in May, 1884, on account of the condition of her left eye, which had been failing for about five weeks. She stated that her health had always been good. Six months previously the menopause had taken place, and shortly afterwards she suffered from a violent pain in her right eye, associated with loss of sight. For this she was treated unsuccessfully by one of the best of our Dublin oculists. The left eye became dim painlessly. She stated that in both eyes the centre of the field was most affected, and that her sight was relatively better in twilight or on a dull day. I found her vision very defective: the right eye counted fingers at $\frac{3}{4}$ metre, and left at 4 m.; left read Jæger No. 16.

The hemeralopia I ascertained as follows:—By darkening my consulting room I so reduced the illumination that I could only read Jæger No. 16 at 2 metres; at this distance and with this illumination Miss S. read Snellen 40. By increasing the light I was easily able to read Jæger No. 6 at the same distance, while the patient's vision was not in the slightest degree improved.

In both eyes tension normal, pupils wide and sluggish, commencing cataracts in the lenses. The right papilla was atrophic, white with very small arteries, and small dark tortuous veins. The macular region was somewhat speckled by what I took to be atrophic changes. The left optic disc was hazy, margins cloudy,

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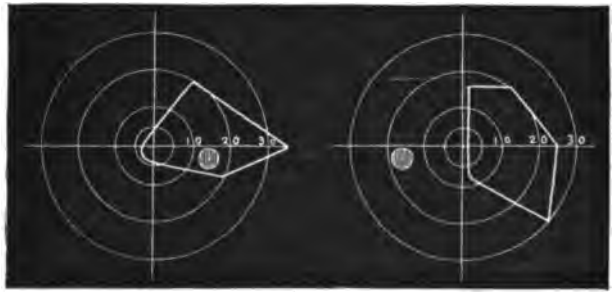
arteries small, veins small, black, and tortuous. There was spontaneous venous pulsation in both retinæ. There was no defined exudation in either retina, but there was in the left a patch of hæmorrhage to the lower outer side of the disc, and a fresh hæmorrhage occurred during the time I had the patient under my observation. The macular region of this eye was of a deep red, and the retina around it appeared somewhat œdematous.

The peripheral limits of the fields of vision were approximately normal, but a large central scotoma could be detected in each field. These scotomata were negative, absolute, so far as colours were concerned, but only relative for white. Colour vision, indeed, was so defective that I found it impossible to take a chart of the fields for colour vision, but it was comparatively easy to map out the fields for white. The scotoma in the right field was pretty exactly circular, and extended to 20° from the point of fixation. That in the left field was more elliptical; it extended only to 10° from the fixation point, and its lower boundary reached at one point only to 5°.

Four months later the fields of vision presented the appearances seen in the accompanying charts. The scotomata are seen to be symmetrical, each extending to the right side of the point of fixation. At this period the peripheral limits of the fields were still normal, but the vision of the left eye had sunk to counting fingers at only 1 m.

The case so far presented nothing of peculiar importance, but when I questioned my patient upon her family history I was informed that her four brothers had all gone blind in exactly the same way, as she termed it, "from the brain."



The space to the right of the fixation point, which is enclosed by the broad white lines, indicates the scotoma in each field.

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Case II.—The eldest brother got sunstroke in India, and also "ophthalmia;" was very like what she is; could only see in twilight; died at age of twenty-three; mind sound to the last; it was, however, of epileptic fits he died.

Case III.—Second brother, while a medical student, aged twenty-one, lost his sight, and became for a while excited; he used to drink, too, to excess. He afterwards became quiet, "settled down." He died of decline. The sister thinks his mind was not affected except by excitement from drink.

Case IV.—Third brother got a sudden fright at death of eldest brother, and lost his sight either then or shortly afterwards. His mind went astray, and he died in a lunatic asylum.

Case V.—The youngest brother got a cold on a journey at age of thirty. He had been in Australia, and the family thought he had passed "the delicate age." After the cold he became unconscious, but he was well in a fortnight—"as well as ever." Then in a month or so his sight went bad just in the same way as the sister's. His mind is usually sound, but he occasionally suffers from religious excitement, and takes dislikes to his family. He has had to be sent more than once to an asylum.

When he lost his sight he consulted a specialist, who told him he had a central scotoma. He is the only one of the family whom I have been able to see except two sisters. His age is now forty-five. With either eye he counts fingers at about 1 m. Ophthalmoscope reveals simple white atrophy of both optic nerves with contracted arteries and veins, the latter slightly tortuous. The presence of a central scotoma could be easily demonstrated in the field of vision of each eye, but no chart could be attempted owing to defective fixation. He appeared to me dull and sluggish in intelligence.

The elder two sisters have good sight, but use glasses in reading. I have seen the eldest sister. Her eyes present nothing remarkable, but she, as well as the one whose eyes are affected, appears to be of a highly excitable disposition. During my interview with them they never stopped chattering, asking questions without waiting for answers, and contradicting each other as to unimportant details, and altogether were in a state of such intense and ill-regulated excitement that it was extremely difficult to arrive at the facts of the family history. What I have put down is the

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result of repeated questioning of each separately. There was no important discrepancy in the accounts they gave, and in the few details where they differed I have followed the narrative of the younger sister, my patient, who seemed the more sensible of the two.

We find, then, in this unhappy family, that of eight children (four girls and four boys) five have suffered from this remarkable affection of sight. Of these five four are males, and all have manifested some sign or other of further nervous trouble. One died of epilepsy, two have been in lunatic asylums, and one suffered from mental excitement attributed by his sister to the effects of drink.

Upon the pathology of this obscure affection I do not presume to enter. Without the evidence of *post mortem* records, to do so would be to travel into the realms of speculation. It is enough to state that the disease is a retrobulbar neuritis, or at least a neuritis of the peripheral extremity of the optic nerve, and that it seems to be complicated with other less obvious alterations in the nervous system.

The treatment I have used has been unsuccessful. Iodide of potash and mercury have been the chief remedies employed. I have not as yet tried galvanism, which seemed in one of Leber's cases to lead to a successful result.

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CHOLERA: ITS ENDEMIC AREA AND EPIDEMIC PROGRESSION, WITH REMARKS ON ITS CLINICAL ASPECTS AND TREATMENT.

By SURGEON-MAJOR HAMILTON,
Army Medical Staff.

[Read in the Medical Section, January 30, 1885.]

In considering a subject like that of cholera, the first great difficulty that strikes the investigator is the number of views, conflicting one with the other, each put forward confidently and with dogmatic assertion, and, no doubt, from the standpoint of the various writers, in perfect good faith as to their correctness.

Each local observer necessarily receives different impressions in accordance with the facts that come within his observation; and thus we have a number of theories, many of them most conflicting, and each upheld most strenuously by their different advocates.

In opposition to these local observers, we have the statistician surveying the facts from a central office, comparing manifold observations as regards numbers, dates of attack, and progression, and from these various data working out a harmonious relation of one epidemic to another, till he has indications sufficient to map out the natural laws and progressive steps of the disease.

Perhaps the simplest illustration that can be given of the conditions under which such observations of epidemics are carried out, will be the case of a great battle, in which there are several reporters present with the troops in different parts of the field.

The incidents of the fight and the conduct of the troops in his immediate vicinity will naturally be reported as observed by each, and, as a consequence, a number of very different views will be put forward. Overhead, however, in a captive balloon, another

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reporter has a bird's-eye view of the entire battle-field, and he is, in consequence, able to give a comprehensive report of the entire engagement. It stands to reason that the account of the reporter who enjoyed a complete view of the battle must be more accurate, as a whole, than that of any of the others, who could merely speak of the conduct of the troops in their immediate vicinity.

It is often said that statistics can be made to prove anything; but, from personal knowledge of the records of cholera in India, I assert that statistics of the army in that country are a marvel of accuracy, and from them have been deduced most valuable data. The late Dr. James Bryden's works on epidemic disease in India teem with information of the most instructive kind, and I have no hesitation in saying that to him we owe the greatest portion of our knowledge of epidemic cholera.

The first striking point in connexion with cholera in India (I speak now of the Bengal province) is the fact that there is an endemic area from which the disease is never absent, even while the rest of the Continent is absolutely free. This endemic area, or, as it may be termed, the manufactory of cholera, is one of the most remarkable places in the world, and a more suitable position for the purpose it would be impossible to imagine.

The great rivers Ganges and Bramaputra, draining a large proportion of the Continent of India, pour their waters into the sea below Calcutta, but, contrary to the custom of rivers to unite and discharge by a single mouth, these great bodies of water split up into innumerable channels that pass through an alluvial delta of many miles in extent.

The Sunderbands, as they are called, may be described as a crowded cluster of small marshy islands, separated by narrow channels, in some places containing brackish, in others fresh, water, and generally under the influence of the tide. These islands have obviously been formed by the deposition of the enormous quantities of earth swept down by the rivers during their periodical inundations. This alluvial archipelago is 158 miles long, by 75 miles wide, and contains 6,500 square miles.

Lying between the Bay of Bengal and the inhabited part of the

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delta, exposed to the influence of frequent heavy rain, alternating with a burning sun, its accumulated and perpetually exhaling malaria, urged on by the S.W. monsoon, spreads disease and death over the whole country. This tract swarms with tigers and other wild animals, whose ravages cause wide destruction both of life and property.

This, then, is the home of cholera, and from this area epidemics take their rise and spread to the extreme north of India, and, under certain conditions, even to the rest of Asia and to Europe.

It will, perhaps, be most convenient to consider next the sanitary conditions of the remainder of the continent of India, over which, from the endemic area already alluded to, cholera becomes epidemic at intervals.

India is a vast alluvial plain, bounded on the north and east by chains of lofty mountains, interposing an icy wall between Hindustan and Thibet. These mountains rise tier after tier to a maximum height of 29,000 feet, with a perpetual snow line at 16,000 feet, and thus present an impassable barrier to cholera in that direction. The plains of India are watered by many great rivers, the principal of which in the Bengal province, the Ganges, flows in a south-easterly direction through a well-marked depression in the plain, commonly alluded to as the Valley of the Ganges, though, to the ordinary observer, this is rather a misnomer, as no actual valley is to be discerned.

The direction in which this vast body of water moves, towards the S.E., is particularly worthy of note, as epidemics of cholera invariably advance up stream, travelling against the current, thus affording a strong argument against the theory of water being the chief vehicle of infection.

I must next briefly consider the general condition of sanitation under which the 250,000,000 of inhabitants exist, and it is briefly summed up as being as bad as it well can be. The excreta of this vast population are daily deposited on the surface of the ground, and, allowing 8 ounces per head per day, it is evident that 125,000,000 lbs. daily, or a total of 20,000,000 tons a year, are spread on the surface of the land. No doubt the area is a great

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one, but the dessication and diffusion by the wind of such an enormous amount of human excreta must be regarded as a great possible source of evil, fouling the water and food supply of the entire country. From experience I can say that the environs of every village are practically an open cesspit, and even in the immediate neighbourhood of our military cantonments matters are little better. The native of India is, perhaps, the most extraordinary anomaly in nature; bound down by caste prejudices in a manner inexplicable to Europeans, he will throw away his food if even the shadow of a stranger or lower caste native falls on his cooking place. Even in the matter of drinking he is so particular that a Brahmin would die of thirst sooner than drink out of the vessel of a man inferior to him—yet this same Brahmin may be seen washing and drinking in a tank, the banks of which are covered with human excrement washed by every shower into the water he does not hesitate to use for all domestic purposes.

Even in Calcutta, the capital of India, and within a few minutes' walk of the palace of the Governor-General, similar insanitation prevails, and the following remarks from the Calcutta Health Officer's Report will prove how extraordinarily apathetic the people of India are to the filthy condition of their water-supply:—"A circumscribed outbreak was reported from Mooktaram Baboo's-street. A narrow passage led to a shallow tank. It was a small reservoir, closely surrounded by huts and houses, whose drainage found its way into it by numerous shallow ditches. The sloping banks were covered with rubbish and refuse of all sorts, in some places heaped into a sort of dunghill. Broken steps led down to the 'water' from two sides, and paths well-trodden and indicating frequent use gave access to it on the other two. I saw a man, lotah in hand, washing his person and dirty black cloth at one of the ghâts, several girls polishing brass dishes with the mire that lay on the brink of the water at the other; an old woman washing rice in a basket at another place, and a young girl bathing in the centre and drinking the fluid. This was turbid and of a dark brown colour, and about 2½ feet in depth." I have touched on these points in order that the sanitary condition of the inhabitants

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may be understood, conducing, as it must, to the propagation and spread of epidemic disease.

It may be asked—What has the great English Government done to remedy this evil? I fear the answer must be, at all events as regards the mass of the population, little or nothing. In Calcutta and other important towns sanitary measures are carried out fairly well, and the water supply in many places has been improved, but as regards the great bulk of the people, they are as much in the dark as they were 1,000 years ago.

Having briefly described the sanitary condition of this great continent, let us consider the history of cholera as observed in its home.

Cholera, then, is endemic in the area I have indicated—the delta of the Ganges. In this tract it is ever present, and when the meteorology of the season is favourable, it overflows its natural habitat and is carried into what we may call the epidemic area. The two great factors necessary for this overflow of the cholera poison are moisture and wind.

During the fierce heat of the summer months enormous evaporation goes on in the Indian Ocean and Bay of Bengal, and when the monsoon sets in, the S.W. wind carries inland the rain-laden clouds destined to convey the necessary moisture to the arid and parched-up land.

These rain clouds are carried rapidly over the endemic area to the Himalayas, and first impinge on a spur projecting into the plains; the monsoon is thus split into two currents—one passing along the Himalayas and the valley of the Ganges, to the north-west; the second passing along the mountains and up the valley of the Bramaputra, into Assam.

So enormous is the rainfall at certain places, that at Cherapoonjee the annual deluge is 600 inches; I have myself seen 34 inches of rainfall in 56 hours at Naini Tal, in 1880, when the great land-slip took place. This great rainfall not only floods the low-lying grounds of the endemic area, but also moistens the earth northwards, as far as the influence of the monsoon extends.

Bryden's theory, then, is, that the rainfall, swamping the low-

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lying endemic area, extinguishes cholera there; and if the prevailing wind be favourable, the disease is conveyed across the continent, or up the Gangetic or Bramaputra valleys.

This cholera, overflowing the endemic area in the autumn, the period of the heaviest rainfall, advances northwards by well-marked steps, and, influenced by the strength and duration of the monsoon, reaches certain points before the commencement of the cold season. This body of cholera dies out in the winter, and, with the exception of a few sporadic cases, it does not show itself again till spring, when another outbreak usually takes place further north. This again dies down, re-appearing when the monsoon is well on, and frequently lasts till September. Put in its shortest terms, "cholera is ever present or endemic in Lower Bengal, and may become diffused or epidemic in any year. When it travels to the N.W. the invasion is frequently confined to a natural area bounded by the line of 80° east longitude.

"This line bounds the area over which the S.W. monsoon blows, and thus the limit of the extension in a westerly direction may be accounted for.

"It never appears in Northern India before the 20th April in any year. Individual cases west of the line of 80°, especially in the last quarter of the year, very constantly precede the appearance of cholera in the following spring. If the upper provinces be affected as an area, cholera will last till the third week in September. Experience shows that a bad cholera in spring is followed by a worse during the monsoon, usually appearing a fortnight after the beginning of the rains. Cholera, epidemic in the east of India, may be entirely diverted from the N.W. into Western India. This takes place during the prevalence of the N.E. monsoon, in the last quarter of the year."[a]

As an illustration of Bryden's theory, I will take the epidemic of 1878-79. An overflow from the endemic area took place about July (Upper India being up to then free). Appearing first at Hazareebagh, it advanced rapidly, showing itself almost simultaneously along all the stations of Oudh and the Gangetic

[a] Deputy Surgeon-General Marston, A.M.D. Report, 1878.

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provinces, between the 10th and the 16th of August, culminating in the outbreaks at Fyzabad and Lucknow. Dying away after the rains, the disease subsided as an epidemic, but the occurrence of some sporadic cases further north, during the cold season, enabled Bryden to prognosticate a further advance of cholera, which he believed indicated an epidemic in spring.

I was at the time Statistical Officer of British Troops with the Government of India, and in November I received a telegram announcing that a soldier, belonging to a battery of Royal Artillery marching up country, had been attacked between Amballa and Meean Meer.

I showed this telegram to Dr. Bryden, and he then affirmed his belief, from that and other sporadic cases, that the Punjab had been invaded; and he was of opinion there would be an outbreak extending up to Peshawar, and perhaps into Afghanistan, in the next hot weather. This forecast was verified only too truly; in the summer of 1879 an epidemic of cholera swept over the Punjab and right into Cabul through the Khyber, carrying off thousands of natives and many Europeans.

It may be asked—What is the practical outcome of these observations, and, admitting this wave-theory to be correct, of what value is the knowledge?

The answer is, that when indications of the advance of the disease into the northern provinces are present, it would undoubtedly save many lives if the troops were removed during the winter from the territory that experience shows will next be invaded.

Unfortunately, political and military exigencies do not at present permit of such a radical step being taken, though, perhaps, with further experience of the course of epidemics, and with the extension of railways, even this movement may be adopted in the future. From a mere monetary point of view a great saving might be accomplished, and, as it is estimated each British soldier costs £200 in India, the saving of even a few lives would repay the cost of transit of the troops out of the line of danger.

As an illustration of the value of Bryden's knowledge of the progress of epidemic cholera, let us look at the summer of 1879, when

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we had a large force in Afghanistan up the Valley of the Khyber. The peace of "Gundamuck" was signed, and the Government was anxious to withdraw the troops for political reasons. Unfortunately at the time cholera was raging in the Punjab, at Peshawar, and even into the Khyber Pass. Most of the troops were camped on high spurs, and were in good health, and now the question arose what should be done? The cholera was evidently advancing up the Pass, and it seemed certain that if the men were marched out into India they would be attacked by the advancing epidemic and would suffer severely. If, on the other hand, the troops were allowed to remain, and were attacked *in situ*, the losses might be greater; moreover, if once attacked they would have to remain in their camps, as almost certainly the native followers would suffer in great numbers, would then desert, and leave the columns immovable for want of carriage. At this crisis the Government of India consulted Dr. Bryden, and this was his advice:—"If you march the troops out they will certainly be attacked *en route;* but, on the other hand, from past experience, there seems no doubt the disease is rapidly extending into Afghanistan, and if the troops are attacked in camp they will then have to stay there till the epidemic ceases, and the mortality is certain to be enormous."

The Government of India acted on this advice, which met with much opposition, and which, to those not so well informed, seemed almost suicidal; the troops were rapidly marched out into India, and lost many men on the way, but, spreading out like a fan in every direction, the disease soon disappeared.

In proof of Bryden's sagacity, it is well known that a violent epidemic of cholera appeared at Cabul in July, and further, that those troops that remained encamped at Lundi Kotal, a few miles up the Pass, suffered severely. Had our army remained to be attacked, there is little doubt their losses would have been tenfold. This, then, is Bryden's theory of cholera—"earth-born" and "air-borne." The theories usually set forth in opposition to this are two in number—one the "water theory," the other the "human intercourse theory."

It would be quite impossible within the limits of such a paper as

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this to enter in any way fully into either of these views, but I may briefly touch on the main arguments in opposition to them.

In the first place, it is remarkable that cholera in India, certainly in the Bengal presidency, always advances up stream—*i.e.*, *with* the wind and against the current; in the second place, years will sometimes elapse between epidemics, though it is quite certain the water supply in the interval is just as bad; and, in the last place, why, if water is the cause, should not epidemics localise themselves where the water supply is foul, and why should an epidemic suddenly cease, though not only is the water as dirty as before, but, in addition, the so-called cholera poison has been added to it from the excreta of the sick?

It is never denied—on the contrary, it is admitted freely by all who have gone into the subject—that foul water will intensify a local outbreak, as will any other insanitary item—*i.e.*, overcrowding, bad ventilation, improper food, sewer gas, &c., but when theorists attempt to explain the advance of an epidemic over a vast continent by means of water, their arguments utterly fail to convince unprejudiced observers.

In an interesting paper by Pettenkoffer, lately published in the *Lancet*, the various well-known instances quoted by the water theorists—such as the Broad-street pumps and the water supply of Fort William, Calcutta—are thoroughly gone into and their fallacy exposed. Pettenkoffer sums up by saying—"The further one investigates the drinking water theory, the more and more improbable does it become. Robert Koch, too, the famous bacteriologist, has hitherto failed to substantiate the drinking water theory, and I feel convinced that the time is not far distant when he will own that he has gone in the wrong direction."

Remarkable evidence, supporting these views, has lately been published by the Cholera Commission that has been investigating Koch's bacillus theory.

Another contribution to the literature of cholera appears in a supplement to the *Gazette of India* of Jan. 24:—"Dr. Koch, it will be remembered, insisted that the comma bacilli which he found in tank-water in Calcutta were the exciting causes of cholera, but his

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theory has been upset by the investigations which the Special Cholera Commissioners from England recently carried out. Dr. Klein, for instance, having called in the aid of a local expert, Dr. D. D. Cunningham, made a careful examination of tank-water, and the results obtained were very striking. *Thus, on November 14th and 16th, cholera broke out in three houses in Jeleapara Lane, inhabited by well-to-do people. The houses were near to a tank, but the people attacked drank the pure and wholesome water supplied to the town by the water-works. The poor people living in the 'bustees' on the banks of the tank used the latter, which was very dirty and teemed with the comma bacilli, but, strangely enough, not one case of cholera occurred in the 'bustees.'* Again, in *Sahib Bagan*, where a *bad outbreak of cholera took place in the spring, which Dr. Koch at once put down to the use of local tank-water, inasmuch as he had discovered the bacilli in it, only one case of cholera occurred in November, although 200 families used the water for washing, bathing, and drinking purposes. Another tank adjoins and is connected with this one, the water running from one to the other, and yet in the 'bustees' about this second tank not a single case of cholera has occurred during the whole of this year. The people drank the water freely, though the dreaded bacilli abound in it.* Surely, after discoveries such as this, Dr. Koch's theory must be put aside as utterly erroneous and misleading."

The question of human intercourse is even a more plausible theory, and though it is admitted that, under certain circumstances, it may be a factor in spreading an epidemic, yet, equally, it will never account for the advance of an epidemic as a whole.

The best illustration of the fallacy of the human intercourse theory is given by the great fair at Hurdwar, on the Ganges. To this point the natives of India troop in hundreds of thousands to attend a fair and perform a pilgrimage. They come from every point of the compass, or, to use a simple illustration, they resemble the spokes of a wheel entering the nave. It is equally indisputable that when the fair is over the pilgrims return to their homes, radiating in every direction.

Cholera may rage at Hurdwar, and the pilgrims, in consequence, will die in large numbers, both there and in the immediate vicinity,

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Diagram illustrating the progress of pilgrims to the Hurdwar Fair in 1879.

Diagram illustrating the departure of pilgrims from the Hurdwar Fair in 1879. The light shading represents the routes on which the pilgrims carried Cholera with them.

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when the fair breaks up and they are returning to their homes. It is evident that, if human intercourse had the influence claimed for it, cholera would spread equally in every direction—in other words, that the spokes of the wheel would conduct the "materies morbi" to its circumference; but is this the case? Certainly not. Taking an illustration from the epidemic of 1879, it spread to the N.W., the N., and the N.N.E., but to the E., the S., and the S.W., there was no spread of the epidemic. Surely, if human intercourse has the power that is claimed for it to spread cholera, the epidemic should have raged equally in all directions, but this is proved not to have been the case. To put it shortly, cholera in India neither radiates nor retrogrades.

Again, to take an illustration from hospitals. In the epidemic of 1867 cholera was treated in no fewer than sixty-seven hospitals, and out of this number the nurses in eight only were attacked, while those in fifty-nine remained quite free. In another epidemic, at Cawnpore, there were cases of cholera under treatment continuously from May to September, yet not a single attendant was attacked. In my own experience, extending over some sixteen years, I have seen six epidemics, and in no single instance was an attendant attacked in the hospitals of which I held charge.

Of course cholera may (and often does) attack a hospital; but the evidence points to the fact that the building itself has become a centre of disease, exactly the same as a barrack will. Indeed, there is rather reason to expect that cholera would attack a hospital in preference to a barrack, because the former is continuously occupied, while a barrack, for many hours daily, is empty. The air in the former should, therefore, be more suited to receive a specific poison in consequence of the greater amount of human excrementitious matter present.

Another point of great interest is the fact that cholera is never carried out of the endemic area by railways to become epidemic further north, unless there is clear evidence of the advance of the overflow in other directions. Trains daily leave the endemic area for the upper provinces, and it is evident that, if human intercourse alone was sufficient to convey cholera, this must frequently happen,

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but in practice it is found not to do so. Again, trains run daily from Meean Meer to Mooltan, and though cholera may rage at the former place, it is hardly ever known to break out at the latter. Mooltan is frequently without rain for years, but when it does fall, it is then, and only then, cholera makes its appearance.

No theory, therefore, of contagion or water infection is adequate to cover the phenomena of an *epidemic of cholera* as observed in India. On the other hand, Dr. Bryden nowhere denied the truth or accuracy of European observations, nor the manifestations of contagious or infectious properties in this disease; and I have frequently heard him admit that the evidence of contagion, in Europe, would lead to the belief that there the disease may take on a typhus form, and thus be more or less contagious.

There is, however, one great advantage to be gained from a propagation of the non-infectious theory—at all events, till the contrary is proved—viz., that while it is right every precaution should be observed regarding the foul linen and excreta of the sick, nervous people are less likely to suffer from the depression that so predisposes to an attack, and nurses for the sick will be more easily procured and have greater confidence, if a belief in the slightness of risk can be established.

The next point to consider is, what steps can we take to ward off an epidemic of cholera. I observe Dr. Cameron, in his able article on this disease in the *Dublin Journal of Medical Science*, Vol. LXXVI., says:—"The most effectual manner in which to exclude cholera from a county would be to prevent the entry of persons and articles from infected districts." I do not know whether Dr. Cameron still entertains these views, but I see he admits that "in practice it has been found impracticable to effect so rigid a quarantine as this."

Pettenkoffer says:—"Even perfect sanitary cordons and quarantine would also be valueless, for the reason that they are commenced too late."

I go further, and believe that, even if commenced in time, and perfect in every way, quarantine has as much power to keep out epidemic cholera as the proverbial pitchfork has to keep back the tide.

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Quarantine may be summed up as impracticable, useless, and vexatious. At the same time, no precautions should be omitted at all ports to examine ships entering; and in the case of cholera having been on board, the vessel should be disinfected, and the crew isolated for at least ten days subsequent to the appearance of the last case.

The next point to consider is the evacuation of infected localities; and here our Indian experience points to the great value of immediately vacating stations and buildings in which cholera has appeared.

I have seen troops attacked by cholera in barracks, and the disease has at once been checked by the removal of the men into tents pitched a few feet from the vacated building. I have even, under these conditions, seen the men occupy the verandahs of the barracks during the heat of the day without injury, but we were always most careful to oblige them to sleep in the tents at night.

The regulations drawn up by the sanitary branch of the War Office contain most comprehensive and intelligible instructions on this head, and I would suggest their being republished for the benefit of the public at large.

Should cholera attack large institutions, such as workhouses, lunatic asylums, prisons, &c., the instant evacuation of the block attacked should be at once carried out; if further cases occur, the entire place should be vacated, the inmates being accommodated in huts or tents till the danger is past. Similar steps should be taken with private dwellinghouses wherever practicable.

In India our action is—first evacuation into tents near the barracks, and if that is insufficient, the troops are marched out to selected camps. If cholera again appears, the camp is moved further off, at right angles to the prevailing wind. As a rule, the regiment is broken up into as many separate detachments as possible, usually limited by the number of medical officers available. So elaborate a system as this could hardly be adopted in this country, but certainly in a modified form the same principle would apply.

It would be superfluous to detail the sanitary measures to be

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adopted on or before the appearance of an epidemic, but I may shortly say that "*mens sana in corpore sano*" ought to be the object aimed at. Ventilation, water supply, personal cleanliness, food, &c.—all should be well seen to, and, in addition, the use of saline purgatives should be strictly prohibited. As regards this point, Sir Ranald Martin indicated its dangers many years ago, and I believe that, during an epidemic, it would be less dangerous to drink daily of polluted water than to take a seidlitz powder in distilled water every morning. Anything that induces a serious purging should be strictly interdicted, unripe fruit and effervescing salines especially. In the epidemic of 1879, among our troops in the Punjab, I know of two well-marked instances of cholera following immediately the use of Eno's fruit salt.

Before I enter on the treatment of cholera, let us consider for a moment what the disease really is, and what are the causes of the symptoms, as without a clear understanding on this point all treatment is merely empirical. Cholera, I believe, is an "infectious disease," not in the manner the ordinary term infection indicates, as illustrated by scarlet fever or smallpox, but it may be defined as a disease "caused by the reception from without of specific infective material into previously healthy bodies, which material acts like a poison." What this "specific infective material" may be, we are at present quite unable to define. Koch's theory of a "comma bacillus" seems to have come to a "*full stop*," and the labours of the Commission, lately sent to India by the English Government, have quite exploded his arguments—*vide* the report of the *Lancet* of 3rd January.

Acknowledging, then, our ignorance of the cause of cholera, let us look to the effects produced by this specific infection on the human body. Briefly, then, there is a stage of premonitory diarrhœa, which, as a rule, is characterised by a rapid invasion, copious loose bilious motions, absence of pain, and apathetic prostration. This premonitory stage of cholera is almost invariably noticed, and it is during this stage that treatment, at all events as regards medicines given internally, is of any avail. The chief danger of this premonitory diarrhœa lies in its painlesss nature, and the apathy which

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seems to blind the patient to his condition. Cholera is associated in the public mind with cramps in the limbs and colicky pains, so that when a person is merely attacked by a simple painless purging, too frequently little or no notice is taken till all chance of checking it has passed away. In such a case the patient becomes the victim of the most terrible disease known to man—malignant cholera. It is impossible to impress too strongly on the public the urgent necessity of at once attending to every case of diarrhœa the instant it sets in, when cholera is epidemic, or its advent expected. In such cases every moment is of the utmost value—even the time lost in procuring medical aid may be followed by a fatal result.

The simplest rules should be laid down, so that on the appearance of an attack of diarrhœa the patient may be placed in the best possible condition to check the invasion of, what may be, an attack of cholera. The recumbent position, warm blankets over the body, hot bottles to the feet and legs, and sinapisms to the epigastrium, are remedies within the reach of all. If to these be added a simple astringent mixture (it is inadvisable for obvious reasons to call it a cholera mixture) of dilute sulphuric acid, tincture of opium, spirits of chloroform and peppermint, and doses are administered every hour or two, there is a reasonable hope of the diarrhœa being held in check. I can say from experience that many cases that would, arguing from analogy, have passed into true cholera, have, by these means, been arrested in time.

I would suggest that, on the appearance of an epidemic, instructions of this nature should be widely distributed, and, in the case of the poorer classes, a supply of a simple astringent should be issued gratis. The cost of such precautions would be infinitesimal in proportion to the advantage gained, and much confidence would be inspired among the population.

If unhappily the premonitory diarrhœa passes into true cholera, we next have a series of symptoms of the most definite and urgent nature. The face becomes blue and cadaverous; the voice degenerates into a whisper; the skin is cold and shrunken; the pulse feeble or imperceptible; the kidneys cease to act; the purging becomes watery (rice water), mixed with flakes of mucus; there is a

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total absence of bile in the evacuations; incessant vomiting sets in; violent cramps attack the limbs; the tongue and breath get cold (generally a fatal sign); and yet, withal, the intellect remains clear, almost to the end.

This condition generally ends in death; but reaction may happily set in, the functions may be gradually restored, and, with great care, the patient may recover.

Let us now consider for a moment what is the mechanical cause of the terrible train of symptoms above enumerated—in other words, what changes within the body are going on.

I omit all mention of what may be the cause of the conditions, for the simple reason that I believe we know nothing whatever on the subject, and the best thing we can do, under the circumstances, is to acknowledge this fact, and hope for enlightenment in the future. Eminent authorities, such as Dr. George Johnston, have put forward various theories, but none of them will bear the light of investigation, and they completely fail when put to the crucial test. What we really observe is an exosmose into the intestinal tract of the watery portions of the blood, and to this cause, I believe, all the observed symptoms are due.

It is sometimes alleged, as an argument *per contra*, that persons die of cholera without having had any purging. This is quite true, but can a case be quoted in which *post mortem* examination has not revealed the fact that the small intestines were distended by the rice water fluid, which, from some mechanical cause probably, had been penned up and not evacuated?

The blood, in fact, is drained of its serum, and to this cause, I hold, all the symptoms may be traced; the diarrhœa, as is evident; the suppression of urine, because there is no water to eliminate; the absence of bile, because the gall bladder has been mechanically emptied, and there is no secreting action of the liver progressing; the lividity of the face and skin on account of the non-arterialisation of the blood, caused by its tarry condition, which renders it unable to circulate in the minute vessels; and the general collapse and loss of temperature, to nervous shock, the result of the above conditions.

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When we come to treat a patient in this state, what is the usual course? As to remedies, there is no known disease in which so many vaunted cures have been put forward, or in which so many drugs have been employed with alleged success. Calomel and opium is the sheet anchor of many; strychnine was used till unfortunate patients died of its toxic effects. Hypodermic injections of chloral, at another time, were believed a certain cure. Arsenic, iron, and every known astringent, has been employed without success. Tourniquets on the main arteries of the limbs were advocated with a view to confining the circulation of the blood-supply to the nervous centres. At one time even plugging the rectum was advocated, and some twenty years ago a circular was sent round to medical officers in India pointing out that this line of treatment had been recommended by an Italian practitioner. Have any of these numerous remedies stood the test of an epidemic of true cholera? The answer is only too evident.

Let us, then, face the truth and acknowledge we know nothing whatever of the causation of true Asiatic cholera, or of its treatment by drugs in the algide stage. The most we can do is not to interfere with Nature's efforts to effect reaction, and, if happily this takes place, to guide matters to a favourable issue.

During an epidemic in India most of those who pass into collapse, certainly nearly all in the commencement of the epidemic, die. In the last great outbreak in India, in 1879, in Southern Afghanistan no fewer than 88 per cent. of those attacked died, and of the 833 British soldiers who suffered from the disease that year, 654 met their death, or a total mortality of nearly 75 per cent. of deaths to attacks—practically, an entire British regiment disappeared in a few weeks, at a cost to the Government of £130,000. As the epidemic subsides, the deaths, in proportion to attacks, become fewer, and towards its close the great majority of the cases recover. This fact is one of the main causes of such a number of different remedies being put forward as specifics. In the commencement of an outbreak practitioners prescribe medicines that bear a repute for their efficacy, yet all the patients die. Other drugs are tried, apparently with more success, till at last nearly all the patients

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recover. It is at once assumed that "post hoc" is "propter hoc," and regrets are loud that such a successful line of treatment had not before been adopted. In the next epidemic it is prescribed with confidence, but, lo, the mortality is as great, or greater than ever.

As I have shown, the death-rate of the last great epidemic in India was as high as 88 per cent., and here it must be remembered the sick soldier was placed under the best possible conditions—conditions it would be absolutely impossible to equal in any civil community.

Skilled medical aid and unlimited nursing was at immediate command, yet of those who passed into collapse but a very small percentage indeed recovered.

What a burlesque on our science it is to see practitioners under such conditions adopting the most opposite lines of treatment. One man orders castor-oil to eliminate a "materies morbi;" another, calomel to restore the secretion of the bile; a third, diuretics, with a view to getting the kidneys to act; a fourth, brandy and beef-tea, to keep up the vital powers—all with the same results. If, now, we would only remember that an exosmose into the intestinal tract has reversed the laws of nature, we would see the inutility (to say the least) of our proceedings. If no absorption is going on, how can we expect our drugs and stimulants to be assimilated—in other words, when an exosmose from the blood is progressing, how can we expect a reversal of this action in favour of our remedies. This is the point that is too often forgotten, and this is what should be our guide in determining our line of treatment.

The simplest proof, perhaps, of this view, is the effect, or rather the want of effect, of stimulants on a patient suffering from collapse. Champagne, brandy, and beef-tea, may be given *ad libitum*, and (supposing them to be retained) without the least effect on the circulation. The voice does not gain in strength; no flush comes to the face, and the pulse is just as imperceptible, or thready, as it was before.

What lesson should be learned from this? I reply that if no absorption is going on it is useless to administer drugs, stimulants, or food, and I go even further and believe that their administration

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may be positively hurtful. Let us take an ordinary case that has passed into collapse, and we will suppose the usual treatment—Calomel, 2 grs.; opium, gr. i.—every hour—with small and repeated doses of brandy and beef-tea.

If Nature comes to the rescue and gets rid of our drugs and stimulants, no harm, perhaps, is done; on the other hand, let us suppose the vomiting has either ceased, or has never set in, several doses of medicine, with some ounces of brandy and beef-tea, have been retained, what will be the condition of the patient should reaction set in? Nature restores the natural action of the intestinal tract, endosmose re-commences, and at the very moment we should be most desirous of keeping the absorbents at rest, they are called on to assimilate so many grains of calomel and opium, and so many ounces of brandy and beef-tea. The result is too often disastrous to the patient, and our well-meant efforts cause a fatal termination from secondary fever. What, then, should be our line of treatment? When the premonitory diarrhœa has passed into the stage of true, or malignant cholera, our efforts should be directed to keeping up the warmth of the body with hot bottles and well-warmed blankets. Sinapisms to the legs and epigastrium should be applied. The cramps may be relieved by frictions with stimulating liniments, or hypodermic injections may be cautiously used; for these, I think, a combination of morphia, atropine, and chloral, is the most valuable. As to internal treatment, what line should we adopt? If medicines and stimulants are not absorbed, why give them to be a possible source of danger hereafter? Endeavour to assuage the terrible thirst by means of ice to suck, and soda-water to drink; and by this proceeding I believe the patient is placed under the best possible conditions for recovery in the event of reaction setting in. Then even our utmost care must be exercised, and only the smallest quantities of easily-assimilated food should be allowed, till by degrees the necessary repair takes place and recovery becomes assured.

By adopting the line of treatment I have indicated, we can, at all events, do no harm; and we shall have the satisfaction of knowing that when absorption recommences, its first effort will

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not be directed to the assimilation of poisonous doses of opium, or other drugs.

I trust I may not be regarded as a pessimist, but I must, in conclusion, confess that at present the outlook, as regards the prevention and treatment of cholera, is dark indeed. Let us, however, not despair, and perhaps in the near future light may be thrown on this, the greatest pestilence that has ever attacked mankind.

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A CASE OF SLOUGHING OF THE RECTUM.

By W. M. A. WRIGHT, M.D. Univ. Dubl.;
Medical Officer, Killiney Dispensary; late Demonstrator of Anatomy, University of Dublin.

[Read in the Medical Section, Friday, February 27, 1885.]

The notes which I am about to lay before you are of a case of sloughing of the rectum in its third stage, which appeared to me to present features of sufficient interest in its history, course, and result, to warrant my bringing it under the notice of the Academy of Medicine.

Case.—On January 22nd, 1884, I was sent for to see a lady, aged sixty-five, a widow. Her history was as follows:—She was the mother of a large family, eight children, and had always led a most active and healthy life, taking great pleasure in outdoor amusements—as gardening, &c. She had only once suffered from any serious illness, when, after the birth of one of her children, for two months she was confined to her bed by an attack of inflamed piles; but ever since the birth of her last child, twenty years ago, she was annoyed by these piles, which never bled, but frequently slipped down while she was at stool, and required to be replaced by her fingers, a little operation which she was generally able to perform without difficulty. She stated emphatically that her bowels had always acted most regularly, and that she seldom or never required to take aperient medicine.

On visiting her I found her in bed, with temperature 100° and pulse 100, complaining of great pain from the piles, which had come down the day before, after a copious stool, and which she had not been able to replace. On examination I found a ring of piles outside the anus, tightly gripped by the sphincter, intensely congested, and very tender. Oiling them, I endeavoured to reduce them, and partially succeeded, but was soon compelled to desist, owing to their swollen condition and to the intense pain the attempt caused. I then prescribed soothing treatment, opium

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stupes, &c., locally, to relieve pain, and opium and belladonna internally. On the 25th the inflammation appeared to have extended to the vagina, the vulva becoming tender, and a profuse leucorrhœa setting in. On the 27th retention of urine came on, and for eight days I had to relieve the bladder by the catheter.

On the 28th she told me that she was suffering from diarrhœa, and the nurse showed me what passed from the bowels—thickish, yellow, semi-liquid fæces, with no sign of mucus, or of solid lumps. On this day the skin covering the buttocks on either side of the anus became red, glazed, and erysipelatous-looking, and felt very tense, the inflammation being much more marked on the right side.

On Feb. 1st the skin gave way on the right side, about an inch from the anus, and through this opening liquid fæces commenced to pass. By the 3rd of Feb. the external opening appeared as large as a threepenny piece, and almost all the fæces passed through it, hardly any escaping by the anus.

On the 4th she regained power over her bladder, and the tenderness from the inflamed piles had so much diminished that I was able, for the first time, to make a rectal examination. On introducing my finger I passed it into the largest mass of impacted fæces that I ever met. Before withdrawing my finger I broke up as much of it as I could reach, and then gave a copious enema of soap and water, passing an O'Beirne tube as far as possible—about five inches. The enema brought away a number of scybala, most of which passed by the fistula. Next day I repeated the same treatment, again breaking up the mass, which seemed unreduced in bulk, with my finger, and then giving an enema. The result was, to use the nurse's expression, "a bucketful." The following day again the same enema and result, and now I could clearly define the internal opening of the fistula. At the right side of the rectum, about one and a half or two inches above the anus, a portion of the wall of the gut had sloughed clean away, leaving an internal opening as large as a half-crown piece.

At this time my friend, Dr. C. B. Ball, saw the case with me, and while examining her was able, with his finger, easily to press some scybala, quite as large as walnuts, through the fistula. He suggested the use of enemata of olive oil and oil of eucalyptus, which proved most effective, but their smell was so much disliked by the patient that I had to discontinue them. By daily

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copious enemata, from 4th to 10th Feb., I at last got the bowels free from the fæcal accumulation, and my patient gradually improved in strength, but still complained much of pain over the gluteal region on the right side, where the skin remained tense and red, until the 14th, when an abscess which had formed under the skin there opened into the fistula, causing a copious discharge of odourless, healthy pus. After the evacuation of this abscess my patient's recovery was uninterrupted, and by the end of March she was able gradually to resume her former active habits.

I last examined her on the 22nd of April, and found the fistula had so closed that it would with difficulty have admitted a small-sized probe; the piles remained external, but their mucous surface had become hardened and insensible to pain. I may add that all through she steadily refused to permit any operative interference.

Remarks.—First, as to the exact nature of the case, there can be little doubt that the primary cause of the acute trouble was the presence of the enormous fæcal accumulation, which had formed without the slightest suspicion of the fact on the part of the patient. My opinion is that the irritation and pressure caused by this mass set up the inflammation of the rectum and of the hæmorrhoids; that the continued pressure caused the sloughing of the bowel, just as the continued pressure of a fœtal head during the second stage of a tedious labour may cause sloughing of the vaginal walls. The rectum having given way, the escape of the fæces into the cellular tissue of the ischio-rectal fossa accounted for the periproctitis, the erysipeloid condition of the skin over the buttock, and its ultimately giving way to form the external opening.

Probably the retention of urine was due more to reflex irritation than to any direct extension of the inflammation to the bladder.

I regret that I was able to make only a very hurried search into the literature of the subject, and could not find any cases at all similar to mine recorded, except in *The Lancet* for June, 1866, where there is an account, by Mr. T. W. Nunn, of two cases of "Gangrene of the cellular tissue of the ischio-rectal fossa," which resembled my case in the erysipeloid state of the skin over the

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buttock, but which differed in that in both cases there was no ascertainable connexion with the rectum, that there was a distinctly gangrenous smell from the cellular tissue, and that there was great constitutional prostration (with brown furred tongue, &c.). While in my case there was a large opening from the rectum into the fossa, at no period was there any more objectionable smell than that of fæces; even the pus from the abscess which formed under the skin of the buttock, and which communicated with the fistula, was odourless; and, lastly, the constitutional disturbance was remarkably slight, considering the gravity and extent of the local mischief and the age of the patient. At no time was her tongue much furred, or her temperature over 101° F.

With regard to the treatment it must be remembered that the patient was most reluctant to permit local measures, and refused surgical interference, relying on the fact that on a former occasion she had recovered from an "attack of piles" without any local treatment except warm stupes. Looking back, there is one thing I regret that I did not insist on—a vaginal examination at the beginning of the case, when, of course, I would have detected the presence of the fæcal mass; but even had the cause of the attack been patent from the very first, efficient steps for its removal could not have been taken until the time when I actually commenced them, when the first intensity of the inflammation had somewhat subsided.

In conclusion, I am happy to say when last I saw my patient she informed me that she felt as well as ever she had in her life. She thought the fistula must have quite closed up, as it gave her no trouble whatever; the piles still remained external, but were smaller, and did not annoy her; that her bowels acted daily and freely, and that she would not allow any operation to be performed on her.

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FOUR CASES OF ACUTE PNEUMONIA OCCURRING IN ONE FAMILY AT THE SAME TIME.

By WALTER G. SMITH, M.D.;

Physician to Sir Patrick Dun's Hospital; King's Professor of Materia Medica, School of Physic, T.C.D.

[Read in the Medical Section, Friday, February 27, 1885.]

For some time past pneumonia has been very prevalent in Dublin, and a considerable number of deaths have been occasioned by it, the mortality having been exceptionally high.

From returns kindly furnished by the Registrar-General, I find that the mortality from pneumonia for the first seven weeks of 1885 represents an average of 8·1 deaths per week, against an average of 5·7 per week for the corresponding period in 1884.

In Sir Patrick Dun's Hospital, during the four months November, 1884, to February, 1885, inclusive, my colleague, Dr. Finny, and I met with an unusually large proportion of cases of acute lobar pneumonia, several of which were of a severe type, and the mortality was extremely high.

In this communication I propose to bring forward a group of four cases of acute pneumonia occurring in members of the same family at or about the same time, and in a way which seemed to offer some analogy to the spread of an infective disease:—

Case I.—On the 29th January, 1885, I was asked to see a young gentleman, aged sixteen. I found him sitting in an armchair, breathing rapidly and with distress, with a frequent teasing cough, and the aspect of being seriously ill. His mother informed me that he had been complaining for two days previously of cough and loss of appetite, and had not slept well, but nothing was thought of beyond a common cold. Early in the morning of the day I first saw him he did not seem worse, but about 11 a.m. he

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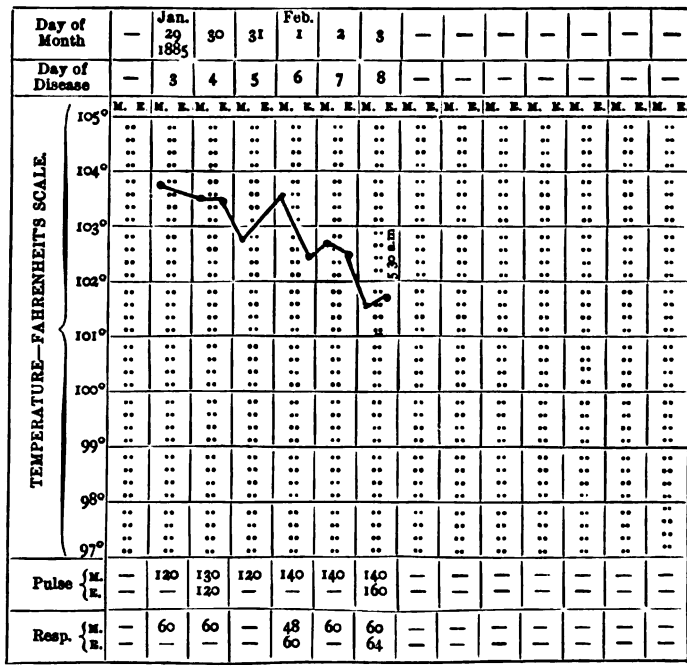
was suddenly struck with a most severe pain in the right side of the chest, which increased in intensity, urging him to bend forwards, and rendering it impossible for him to lie down. At 1 p.m. T. 103·8°, P. 120, R. 60. He was got to bed, and, upon examination, the lower lobe of the right lung was found to be in a state of hepatisation. During the next two days there was no abatement

CHART OF TEMPERATURE, &c. (Case I.)

Name, E. B.; *Age*, 16; *Disease*, Pneumonia; *Result*, Death.



of the symptoms, the respirations were unchanged, and a fair amount of viscid rusty sputum was expectorated. On the succeeding day, *i.e.*, the sixth day of his illness, it was discovered that the base of the left lung was involved. The urine was albuminous, and he rambled constantly. From that time the pulse never fell below 140, and the respirations seldom, if at all, below 60. He was a lad of powerful build, fond of bicycle exercise, and had been in vigorous health previously. Abundance of nourishment was taken,

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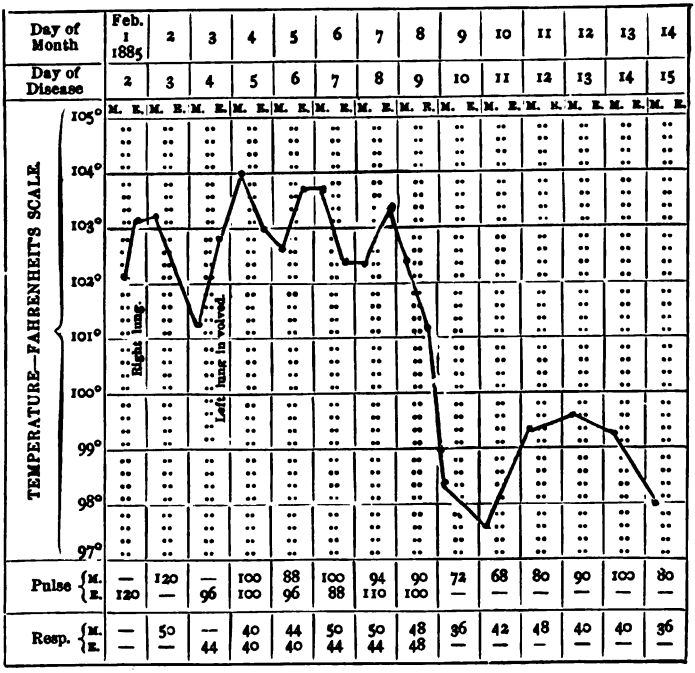
with a due amount of stimulants, and he made a brave fight for life; but on the night of the eighth day of the illness his strength began to ebb, the dyspnœa intensified, and, after a scene of noisy delirium and of violent struggles, painful to witness, the end came.

Case II.—On the sixth day of this lad's illness, *i.e.*, February 1st, I was requested to see his brother, aged thirteen years, who was complaining of a pain in the right side of his chest from the day previous (January 31st). When examined, it was found that he had acute pneumonia of lower lobe of right lung. T. 102·2°–103·2°, P. 120, R. 50. In the course of the next afternoon and

Chart of Temperature, &c. (Case II.)

Name, A. B.; *Age*, 13; *Disease*, Pneumonia; *Result*, Recovery.



succeeding morning his temperature fell to nearly 101°, but a rise set in during the afternoon of the fourth day, coincident with implication of the lower lobe of left lung. The T. rose to 104°, and he looked more seriously ill; yet, singularly enough, the pulse

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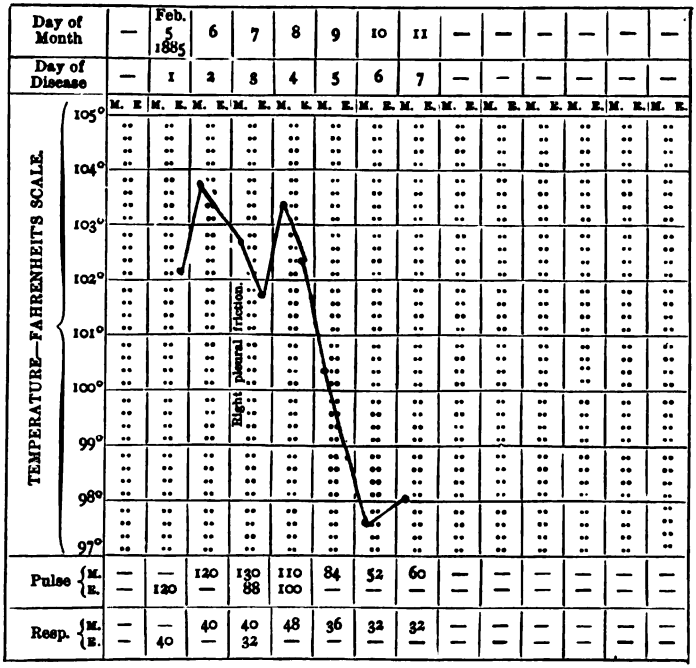
did not exceed 100, nor the respirations 40 in the minute. The pain in the side required the use of morphia hypodermically more than once.

Of his case, it is only needful to say further that no material change occurred in the symptoms and physical signs until the evening of the ninth day, when crisis set in, marked by a sudden fall of 5° in temperature, with amelioration in his general condition. The base of the right lung did not, however, clear up for several days, and the respirations remained, for about a week, considerably above the average. Thenceforward he made an excellent recovery.

CHART OF TEMPERATURE, &c. (Case III.)

Name, W. B.; *Age*, 7; *Disease*, Pneumonia; *Result*, Recovery.



CASE III.—On the 5th of February, the youngest boy, aged seven years, looked poorly, and his temperature was found to be 102·2°, pulse 120, and respirations 40. Next morning the temperature

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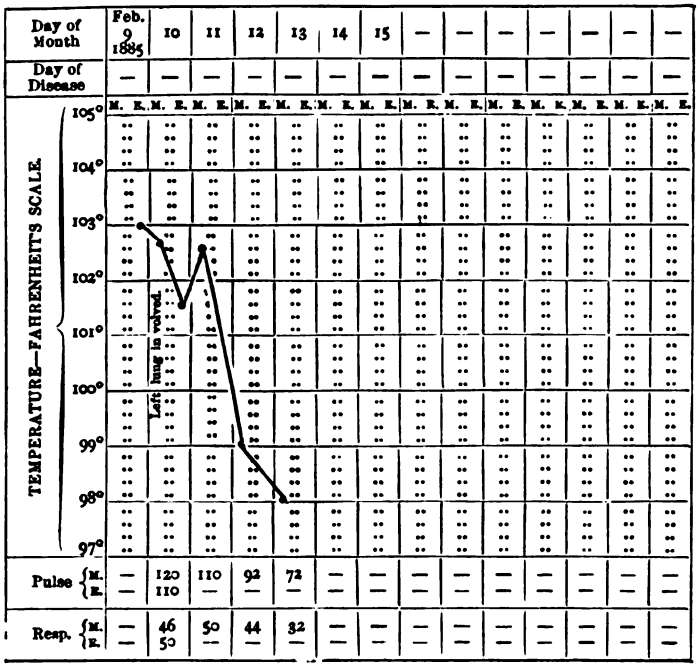
rose to nearly 104°, and the physical signs of pneumonia of the right base declared themselves. His case presented no features of severity. A crisis occurred on the evening of the fourth day, and was satisfactorily completed within twenty-four hours.

Chart of Temperature, &c. (Case IV.)

Name, Miss K. B.; *Disease*, Pneumonia; *Result*, Recovery.



Case IV.—On February 9th, I was asked to visit an elder sister of these boys. She had been ailing for some time with a harassing cough, and was looking badly; but, as long as she could, she had put off the evil day of seeing the doctor, and refused any attention until she became so ill that she was, perforce, compelled to give up. That evening her temperature was 103°, and she was racked with pains in the chest, side, and hips, necessitating the use of morphia hypodermically. Next morning her pulse was 120, respirations 46, and the base of the left lung was found to be consolidated. The real duration of her case is uncertain, but on

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the fourth day after I had seen her the temperature fell to normal Some time, however, elapsed before she could leave her bed, or before convalescence could be said to be established.

Lastly, I should mention that another of the boys, aged twelve years, was ailing, on February 13th and 14th, with slight malaise and pyrexia up to 100·2, yet without any local manifestation, and he forthwith recovered.

From a clinical point of view we may note, first, the marked differences which these cases exhibited in their course and manner of onset; and, secondly, the mode of death in Case I. Cardiac failure is, I think, in many cases, the direction in which danger chiefly lies, and hence the maintenance of the heart's vigour should be uppermost in our plan of treatment; for patients often die from this cause who have abundance of breathing area still in their lungs, and unexpected death is apt to occur in the stage of gray hepatisation when fever has subsided and the patient seems in a fair way to recover. In all the above cases the base of the lungs was attacked. It is commonly taught that the lung inflammation is of a graver type when the upper third of the lung is attacked, but the conclusion deduced from the data collated by the Collective Investigation Committee[a] is in conflict with this opinion. As one result of their labours, the Committee report that, in regard to the upper portion of the lung as a seat of pneumonia:—1st. This locality is as favourable for the patient as any other (in its duration and mortality, indeed, it is more favourable than most). 2nd. No tendency is observable on the part of patients of phthisical family to exhibit pneumonia at this seat.

The question of main interest which is suggested by examples of multiple cases of pneumonia is in reference to the ætiology of the pneumonia. It may be asked—Are they instances of epidemic pneumonia, or of infectious pneumonia imported into a house, and spreading from person to person, or are they illustrations of so-called pythogenic pneumonia occasioned by sewer gas, fæcal accumulation, &c., and liable to affect several individuals so exposed?[b]

[a] Report on Pneumonia. Vol. II. July, 1884.

[b] Cf. Drs. Grimshaw and J. W. Moore. Dubl. Journ. Med. Sci., May, 1875.

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In reference to my own group of cases, I should state that the boy who died was tended, day and night, with extreme solicitude by his mother, and she remained in good health then and since. The sanitary state of the house is not above suspicion. It is old, and in bad repair; rats enter freely from it into the basement of the adjoining house, and I noticed, every time I passed through the hall, a foul heavy smell, to which I called attention.[a] There is no water-closet near any of the sleeping apartments.

On the other hand, it ought to be mentioned that the inmates had lived in the house for several years, and had generally enjoyed good health, and they were not, apparently, in a depressed state of health prior to the outbreak of pneumonia. About a year ago I attended one of the younger boys for a mild attack of continued fever, probably enteric fever.

In conclusion, it appears to be borne out by our present state of knowledge that a process of pulmonary inflammation (with enormous exudation) may originate in more ways than one—sometimes, perhaps, as the direct result of injuries (traumatic pneumonia), sometimes as the local expression of a constitutional febrile affection, and sometimes as the result of bacterial infection.

[a] Curiously enough, the Collective Investigation Committee remark upon this point that a comparison of the houses and districts where sanitation was reported "good" with those in which it was "indifferent" or "bad" brings out the result that the *lowest* mortality coincided with the *worse* sanitation. The 126 instances exhibiting bad sanitary conditions had a mortality of but 16, or 1 in 8, while the 144 under good sanitary conditions had a mortality of 26, or 1 in 5½ (Report, Vol. II., p. 35).

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HYPERPYREXIA IN RHEUMATIC FEVER.

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THE temperature in rheumatic fever does not, as a rule, exceed the limits of pyrexia, and is usually under 102·5° F. It may occasionally reach 105°, but that it should persist for many hours at, or that it should exceed, this range is quite the exception. Nevertheless, acute rheumatism is one of the few diseased states in which the fever heat may sometimes reach 107°–108°, or even 110°–112°, and in these instances the state of hyperpyrexia becomes a source of the greatest danger to the life of the patient, and *that* in direct proportion to its height; so that if the temperature reach 108°, there seems to be little prospect of recovery, although the worst expectations have not been fulfilled in one or two recorded instances of such great hyperpyrexia.

The following case (reported by Mr. Robert G. Patteson, Resident Pupil) of exalted temperature in acute rheumatism was recently under my care, and illustrates many points of clinical interest bearing upon this rare and unaccountable complication:—

CASE I.—*Acute Rheumatism; moderate Fever; great Arthritic Pain; Sleeplessness; no relief from the use of Salicin; Delirium; Hyperpyrexia on ninth day; Temperature brought down by Iced Cold Pack; Recurrence of Hyperpyrexia on eleventh day; Heart failure and Death; no Peri- or Endocarditis.*—Richard S., aged twenty-five, a groom by occupation, was admitted to hospital on Monday, 17th of November, 1884, suffering from a severe attack of rheumatic fever of three days' duration. His illness began suddenly on Saturday forenoon with a pain in the back, which next attacked the ankles, and travelled thence up the legs. He has never had any

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previous illness, and none of his family had, to his knowledge at least, ever suffered from acute or chronic rheumatism. He attributes the present attack to wettings received in discharge of his duties.

On admission his temperature was 101·2°; pulse, 120. On being placed in bed he sweated a good deal. The bowels were constipated, and were freely moved by a simple enema. The tongue was covered with a thick "blankety" fur. The limbs, as far as the hips, were encased in cotton wool and flannel rollers. He had pain and difficulty in micturition before admission, but afterwards was quite unable to pass any urine. Hot stupes over the suprapubic region were tried without avail; and at 11 p.m. a catheter had to be passed to relieve his distress, which was not due to stricture. As he had not slept for two nights previously, a hypodermic of morphia was administered (m 3, inj. morph. hypoderm. B.P.).

Nov. 18th.—Patient had a restless night, and slept but little; pains no better. He was ordered salicin—20 grs. every second hour—and was placed between blankets. The retention of urine was overcome by hot fomentations, but he has still great pain and hesitancy in micturition. Towards evening the rheumatic pains attacked the arms and shoulders, which were swathed in wool and flannel. He was very restless, and had not slept the previous night under the opiate. The hypodermic of morphia was repeated.

19th.—The patient slept fairly well, and is passing his urine more freely this morning. There has been no marked fall in temperature—100·7°; pulse, 124. The quantity of salicin was increased to 30 grs. every second hour, the effects to be carefully watched. He sweats profusely, and requires the wool covering the chest to be frequently changed. The heart is fairly strong, and there are no evidences of organic mischief. There is a good deal of effusion into the right knee, but he does not complain of any severely localised pain. 3 p.m.—The salicin seems to be agreeing well; temperature, 100·8°; pulse, 108. At 9 30 p.m. the temperature had risen slightly, and the pulse was quicker than in the morning, but there were no indications for stopping the salicin. A hypodermic of morphia (gr. $\frac{1}{4}$) was administered.

20th.—No marked change this morning, the temperature still continuing high. The salicin was stopped and replaced by pulv. ipecac. co. gr. 5, twice during the day, and gr. 10 at bedtime. The sweating has stopped, but the whole surface of the trunk

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and arms is covered with sudamina. There is less difficulty in passing urine. Towards evening, as the opium failed to relieve the pains or to cause sleep, and the pains were so violent, causing him to call out so loudly, ⅓ gr. of morphia had to be administered to allow him and the other patients to sleep. There was slight delirium during the night.

21st.—Morning temperature about the same; pulse rather quieter. The opium was ordered to be continued during the day. Tongue dry and dirty. Complains of pains being worse than ever. Can now pass urine freely. 11 p.m.—Still very restless, impatient, and peevish, and in great pain. Temperature, 104·2°; pulse, 140. A hypodermic of morphia (gr. ¼) was given. Careful examination of the heart was frequently made with negative results, as far as organic disease is concerned. The urine, both now and on several subsequent examinations, was free from albumen.

22nd.—Slept a little during the night. Temperature considerably lower this morning—102·2°. His bowels had been confined the last two days, so he was ordered a haustus oleosus, after which they were moved four times—the first motion solid, the later ones fluid. The temperature still remaining high, quinine was ordered—5 grs. three times a day, alternating with a pill containing 1 grain of the aqueous extract of opium. The heart still remains unaffected, but there is a good deal of effusion into the knee-joints, the surface pitting on pressure in the neighbourhood of the knee. He is now again bathed in profuse perspiration, and the whole body is covered with sudamina (miliaria rubra et alba). His aspect is sallow and careworn, and he rambles at times. The urine and fæces are discharged involuntarily. Towards evening he was somewhat quieter, and for the first time since admission morphia was not administered.

23rd (ninth day).—Passed a restless night, was very delirious, trying to get out of bed. Forehead and cheeks flushed, of purple hue. Temperature, this morning, 103°; pulse, 116. Tongue dry and thickly coated. Ordered quinine (grs. 5), with hydrobromic acid (3½), three times a day, and the pills of opium as before. He rambled a good deal at times during the day. [On this day he reminded me very much of a case which I shall relate further on, and which ended fatally after wild delirium.] At 6 p.m. his temperature being 104·4°, he was given the second dose of quinine. At 8 p.m. the patient's condition was very

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serious, and he seemed fast sinking—was very delirious, and during the day had passed everything under him. At 8 30 p.m., the temperature still rising, he was given 15 grs. of quinine with hydrobromic acid; about 9, his temperature having risen still further (105·4°), cold stupes and ice-caps were applied to the head, but without effect. At 10 p.m. Dr. Finny came down, and an ice-pack was applied with most marked and beneficial results. Towels wrung out of water, in which stood a block of ice, were applied *seriatim* to the head and neck, thorax, abdomen and legs, and lastly to the back, the patient being turned on his left side; and then, by the time the last towel was applied, those on the head and chest were warm, and required to be again changed, and so on. The packing was begun at 10 15 p.m., at which time the patient's temperature was 105·5°; the dyspnœa was excessive, and the pulse 160 per minute. The ice-pack was continued half an hour, with the following results:—

10 30 p.m.—Temp., 104·8°; pulse, 160; resp., 44. 10 45.—Temp., 103·8°; pulse, 144; resp., 44. 11 p.m.—Temp., 101·5°; pulse, 132; resp., 36.

The benefit to the patient was magical. From being delirious, talking loudly, and calling out, he became rational, and said he felt better; his pulse was quieter and stronger, and his respirations, which had been very noisy, much slower and less laboured. A pericardial friction sound, which was audible in the nipple line in the fifth left costal interspace very distinctly when the heart's action was excited before the packing, was hardly audible afterwards. At 11 30 p.m., half an hour after the pack was stopped, the temperature dropped to its minimum, 100·4°; pulse, 128; respirations, 28. At 12 30 the temperature continued the same; at 1 15 a.m. it had risen about a degree to 101·6°; at 2 it remained the same; at 3, 4 30, and 6 a.m., the thermometer registered 102°, and at this point it remained constant for some time.

24th (tenth day).—The good effects of the packing remain, and the patient is much quieter and more rational, though he still rambles at intervals, and has not lost the purple-red blush on cheeks and forehead. He continues to take a fair amount of nourishment, and was ordered an egg beaten up with a little wine. Evening temperature, 101·8°. A rustling sound at the apex is the only physical sign noted.

25th.—Had a restless night, and was very delirious. Tempe-

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rature, 101°; pulse, 128. Quinine was ordered in 3 gr. doses with tinct. iron perchloride (m. 12), three times a day. He continued restless and rambling during the day. In the afternoon he saw his wife, and shortly afterwards became very delirious. At 4 p.m. his temperature had risen to 104·1°; pulse, 148, and very feeble; respirations, 76. At 5 p.m. an ice pack was administered, which reduced his temperature 2°, and to a certain extent relieved the dyspnœa, though its effects were only temporary. A loud, creaking, pleuritic friction sound was audible to-day in the seventh costal interspace, about two inches external to the nipple line on the left side. The results of the second ice-pack were as follows:—

5 5 p.m.—Temp., 104·1°; pulse, 150; resp., 76. 5 20.—Temp., 103·2°; pulse, 144; resp., 72. 5 35.—Temp., 102·1°; pulse, 136; resp., 60.; stopped ice pack. 6 15.—Temp., 102·5°; pulse, 148; resp., 68.

The effect on pulse and respiration was only very temporary, the dyspnœa even becoming as urgent as ever. At 10 p.m. the temperature had risen again to 103·5°, and the patient was manifestly sinking rapidly. At 12 the temperature was 105°, and the pulse 172; heart feeble and fluttering. An ounce of whisky was administered, but without any marked effect. He was now wildly delirious. At 2 a.m. the temperature registered was 107°. At 3 it rose still further, to 108·4°, and the delirium gave place to convulsions of a very violent type. At 3 30 a.m. he died.

Autopsy, twelve hours after death.—A hurried and limited examination led to the following results:—There was no pericardial effusion, and the parietal pericardium was smooth and healthy. The lungs were, to all appearance, healthy, and were non-adherent, but were not removed from the body. There was no pleuritic effusion; no evidence of pleuritis on the visceral pleura. The heart was of normal size, and in normal position, and on its surface a number of "ink" spots were noticed. There were none on the parietal layer of the pericardium. The right side was comparatively empty of blood, only containing a few soft, shreddy clots. The wall of the right ventricle was hypertrophied. The foramen ovale was more patent than is often found, but not sufficiently to allow regurgitation. The left side was normal. There was no evidence of endocarditis. There was no arterial degeneration. The other organs, including the kidneys, were healthy, but deeply congested.

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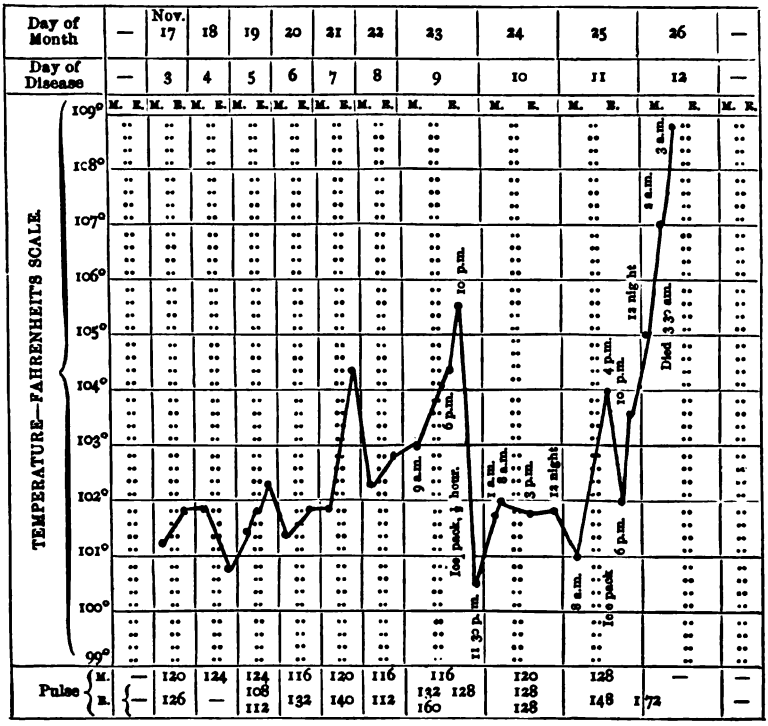
The fluid effused into the knee-joints contained a small quantity of pus. The brain was not removed.

Chart of Temperature, &c.

Name, R. S.; *Age*, 25; *Disease*, Acute Rheumatism—Hyperpyrexia.



In July, 1881, I had a similar case of hyperpyrexia, and, although I have published it *in extenso* in the *British Medical Journal*, Vol. II., 1881, p. 932, I will ask your indulgence while I shortly summarise it:—

Case II.—*Acute Rheumatism; Pericarditis on seventeenth day; Hyperpyrexia and Coma on eighteenth; Iced Cold Pack; Immediate and Permanent Benefit; Disappearance of Friction Sound.*—The patient, thirty-seven years of age, who was a mother of five children, had been a fortnight ill of well-marked arthritic rheu-

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matism when she came under my care. During the first three days in hospital there was nothing to point to the case as being anything unusual. Pulse was 100–108; temperature, 101·8°–102·4°; the skin was perspiring, and the treatment consisted of salicin, in 15-grain doses, every third hour, and the joints were packed with wadding. On the fourth day a slight pericardial rub was detected. The temperature had gone up rapidly to 104·4° between morning and evening, and on the following morning it stood at 104·6°, and pulse at 114. Quinine in 5-grain doses was now administered every three hours, but without reducing the fever. At five o'clock that day the pulse was 136, and the temperature had run up to 107°, and seemed inclined to rise still further, being 0·2° higher twenty minutes after five o'clock.

The patient was partly unconscious; was wandering, complaining of no pain, while her face and lips were flushed, of a purplish hue, and her breathing was shallow, loud, and rapid.

Recognising the danger of the exalted temperature, I at once proceeded to reduce it in exactly the same manner as that already described, and with like immediate good effect. The temperature began to fall at once, was a degree lower in ten minutes, and in forty had fallen to 103·8°, while the pulse had sunk to 120. The flushed appearance passed away, the breathing was deep and natural, and the patient seemed drowsy, and complained of feeling cold. The cold applications were then removed, and at 9 o'clock p.m. (*i.e.*, four hours subsequently) the temperature had sunk to 100·6°, and pulse to 108. On the next day the morning temperature stood at 102°, and the pain was again complained of in the right ankle. The further course of the case was subfebrile, the evening temperature never passing 101·4°, and the patient made a complete, though tardy, recovery.

There is but one other point in this case to which I would refer, and that is the pericardial complication. An adventitious sound was noticed, on the day before the hyperpyrexia, at the base of the heart in the left parasternal line; it assumed a distinctly frictional character the day following; and after the cold packing it was still audible, but less marked, and presystolic in time. Twenty-four hours subsequently this physical sign disappeared, and was not followed by evidences of either effusion or endocarditis.

The foregoing cases are both typical examples of the peculiar state in which hyperpyrexia is the prominent symptom. Before,

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however, addressing myself to this special point, I beg to make a digression, and to call attention to the friction sounds of pericarditis which were heard in each case, especially in the last case. I felt so sure of the diagnosis that I should have been much surprised if the pericardial sac had been found free from inflammatory exudation of lymph, and yet by the light of the *post mortem* revelations of the first case I am compelled to ask myself whether, after all, can a frictional murmur be produced by other causes than by pericarditis, or can it exist in the absence of exudation? Dr. Stokes[a] says on this subject of friction sound as a sign of pericarditis:—"From the rarity of death in the very first stages of the disease, it becomes difficult to declare that a merely dry state of the membrane (as Collin asserts to be the case) will not suffice to produce the sign" (friction), "and there seems no reason why it should not do so" . . . . "Should the exquisitely smooth surfaces of the serous membrane, under the influence of inflammation, become merely dry, it is almost certain that some frictional phenomena would be developed, particularly in the pericardium, where the membrane is pressed upon by the comparatively firm and unyielding mass of the heart."

Were these two cases, then, instances of pericardial inflammation in the very early stage of dryness of the serous membrane, and was the inflammatory change checked by the revulsive action of the cold application, such as we see in local inflammations of limited areas?

A very similar case of rheumatic fever, with hyperpyrexia (108·8°), in which wet packing and ice was applied with immediate and permanent good results, is recorded by Surg.-Major Quinton, Medical Staff.[b]

As, doubtless, many are aware, within the last two years a very valuable inquiry has been made into the subject of hyperpyrexia. A committee, consisting of Drs. Southey, Weber, Ord, F. Taylor, and Coupland, was appointed by the Clinical Society of London to investigate and to report upon the causes, consequences, and treat-

[a] Diseases of the Heart and Aorta. Pp. 8, 9.
[b] Brit. Med. Jour. 1884. Vol. I., p. 1145.

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ment of hyperpyrexia in rheumatic fever, and other acute febrile diseases. They submitted an intercurrent report on May 28, 1882, in which they limited their remarks to an analysis of 67 cases of rheumatic fever collected from various sources, and mostly unpublished. Although they have not as yet completed their labours, and reported finally, there is a great deal of most useful information to be derived from their report, and of this I have availed myself in my comments on this subject.

A statistical comparison of the number of cases of hyperpyrexia with those of acute and subacute rheumatism admitted into the Middlesex Hospital between 1869-1880, shows that of the 1,300 cases treated during those years there were but 22 in which hyperpyrexia was present.[a] It is further noticeable that hyperpyrexia occurred more often in some years than in others, and that of the 67 cases which formed the basis of their Report, the Committee found that 81 per cent. occurred during the five years, 1873-77; and that rheumatism, although it was unusually prevalent in those years, was not proportionately so.

The mortality of these cases of hyperpyrexia is very considerable, as it seems to prove one of the chief causes of death in acute rheumatism, which ordinarily is a disease of favourable prognosis, so far as a fatal termination is concerned. Thus, of the 67 cases referred to above, 33 ended fatally, while the death-rate of acute rheumatism, during the same period, and exclusive of cases of hyperpyrexia, was only 1·8 per cent. In my own experience, I have lost but two patients suffering from acute rheumatism. They were both instances of great delirium and cerebral disturbance, and the temperature in one reached 108·4°. It was possibly high in the other also, but of this I cannot speak positively, as accurate thermometric observations were not taken during the fourteen hours preceding death.

Hyperpyrexia seems to occur frequently in cases of acute rheumatism in which complications have been noted, although the complications are none other than those we are accustomed to associate with that disease—viz., pericarditis and endocarditis,

[a] Report on Hyperpyrexia, Clinical Society's Proceedings, 1882.

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while pleurisy and pneumonia may be, not uncommonly, present in the hyperpyrexial state.

Dr. Francis Sibson[a] has collected 37 cases in which the temperature was excessively high, and about half that number were complicated by pericarditis. As pericarditis occurs but once in every five or six cases of rheumatism, it appears from this that the presence of pericarditis in a case of acute rheumatism increases the chance of the occurrence of hyperpyrexia with delirium and coma in the proportion of four or five to one. "It must not be lost sight of," he says, "that, as a rule, cases of acute rheumatism with pericarditis are in all respects worse than those without it, and that not only at the time of pericardial inflammation, but usually also before it. It becomes therefore a question whether or not the same severity of the acute rheumatism itself, which brought the pericarditis into existence, brought also the excessively high temperature with its attendant delirium and coma into existence, the two affections being affiliated and due to a common cause."

While, therefore, it may be assumed that, should complications exist in acute rheumatism, hyperpyrexia is relatively more likely to ensue, at the same time it should be distinctly affirmed that there is nothing in the onset, course, or type of the attack of rheumatism from which we can predicate the certainty of the temperature rising beyond its usual limits. It has been generally held, and one of the most current views is, that, should articular pain suddenly subside, and sweating be arrested, the occurrence of hyperpyrexia is to be expected, and it has been suggested that the latter symptom (arrest of sweating) may be the chief ætiological factor of the exalted temperature. Dr. Bristowe[b] writes—"The skin not only, as a rule, ceases to perspire profusely, but often becomes dry and harsh, and thereby some poisonous matter, which developed in the course of the disease, becomes retained in the blood and produces the hyperpyrexia."

Unfortunately for the verification of this theory, it was demonstrated that of the 67 cases referred to, sweating was distinctly

[a] System of Medicine. Reynolds. Vol. IV., pp. 270, 271.
[b] Practice of Medicine. Fourth edition.

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present in two-thirds, and sudamina were noted in 22, and that a dry non-perspiring skin was present in not more than one-fourth of the cases. In both my cases sweating existed immediately preceding the occurrence of the hyperpyrexia, and sudamina were present in one.

It seems, however, that a closer connexion exists between cerebral symptoms—the most common being delirium—and hyperpyrexia than any other complication; that they may precede the onset of hyperpyrexia, accompany or follow close upon it; and that, so far as our knowledge goes, those instances in which the delirium precedes the high range of temperature are apparently the most serious, since of the three classes a greater number of deaths have been recorded under this variety than under the other two combined.

It should, however, be borne in mind that occasionally delirium may be wanting, while the temperature may reach 107°, and recovery has taken place; and, again, delirium and death have marked other cases where the temperature never reached any unwonted height.

The following case is one in point on this branch of the subject, exemplifying extreme delirium, coma, and death, without any cardiac complication, and without a recorded high temperature:—

Case III.—*Acute Rheumatism; Limited Arthritis with Purulent Effusion; Active Delirium; Rapid Pulse; Death on seventh day by Coma; no Peri- or Endocarditis.*—A patient was admitted on December 29, 1879, into the City of Dublin Hospital under my care. He was unable to walk, and he hobbled into the waiting-room on two brooms, which he used as crutches. He appeared much exhausted by his exertions, and his face was covered with profuse perspiration; the sweat was of a decidedly acid reaction. He complained of severe pains in both legs, but particularly in the left knee and ankle. The knee was swollen. The pulse was 140 as he sat in the waiting-room, and afterwards (when I saw him in the evening, after he had lain in bed for six hours) it sank to 120; it was full, but not so bounding as is usually found in rheumatic fever. Temperature, 102°. He was bathed in perspiration over every part of his body, and a strong smell of

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sweat was noticeable. His tongue was broad, thickly coated with yellow-white fur, but clean at the tip and edges; it trembled somewhat on being protruded. There was no murmur over the heart, nor other adventitious sound. The symptom most remarkable was a curious restless look about the eyes, and a nervous quick way in which he replied to questions. As the pupils were dilated, and in the absence of photophobia and vomiting, or other evidence of surface irritation of the brain, and as the history pointed to alcoholic excess, the idea of meningitis was excluded, while particular attention was directed to the heart.

His history, as supplied by himself, was that he was a messenger, aged twenty-nine; that on Christmas Day (five days before admission) he received a severe wetting, when he felt cold and shivering, but had no severe rigors at any time, and that he did not lie by till his admission, although his leg pained him a good deal. He did not sleep the night after admission, although he had a sleeping draught. On the following morning, though he complained of no headache, he seemed more excited, and was not perspiring so much; his pulse was 152, and not so full; temperature, 102°. No other joints were engaged. The heart's action was excited, but no murmur or increased area of dulness could be detected.

During the day he became more and more excited, rambling and talking, and in the evening he was in high delirium of the busy type, dealing with his occupation, and he had to be held in bed to prevent him getting out. His face was flushed, he tore off all dressings and applications, and he seemed not to feel pain in the leg. On examining the knee about 11 30 p.m., I found effusion into the joint and a brawny swelling of the whole left thigh. I again examined the heart, but found no murmur; it was beating at the rate of 160. About midnight the delirium and noise diminished, a dusky and then a livid hue took the place of the flush in his face, and he gradually sank, and died at 1 30 a.m.

In treatment he had altogether taken about 120 grains of salicylate of sodium and 40 grains of salicylic acid.

Post mortem examination was made the following day, and the heart was the only organ which I was permitted to examine. I, however, tapped the left knee-joint, and drew off an ounce and a half of purulent fluid. From the bladder I got a small quantity of the urine, which exhibited a very decided amount of albumen, to one-fourth. The pericardium was healthy, containing a small

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quantity of serum which was not purulent (as tested by the microscope), but on the heart, towards the apex and over the back of the left ventricle, there were numerous inky extravasations, such as are found in death by suffocation. There was no valvulitis or ulceration, and the right side was full of black clot. The lungs, however, were not hypostatically congested, nor in any sense hyperæmic.

In the foregoing case the most prominent symptom was the delirium, which occurred without pericarditis or endocarditis; and, so far as our thermometric observations went—for, unfortunately, owing to the patient's extreme restlessness and violence, they were omitted the last day of his life—the temperature had not exceeded 102°.

The similarity of the general symptoms, nervous and thermometric, as well as their mode of onset, and their improvement or modification by the external application of cold to those produced by sunstroke or heat apoplexy, is so striking that one might, with Senator,[a] reasonably assume that all the grave symptoms are merely consequent upon increased heat of the body. The stern tribunal of facts disproves, however, the correctness of this view, since, as just now shown, a high temperature may exist without cerebral disturbance, and severe delirium, ending in death, may occur without, or may precede the advent of, hyperpyrexia.

A further point of great interest on this subject of the causation and intimate relationship of the delirium and the high temperature, and one which the cases I have read exemplify, is the absence of distinct visceral lesions in a very large number of the fatal cases in which *post mortem* examinations have been made. Pericarditis, often along with endocarditis, or endocarditis alone, is that most usually, but by no means invariably, found; while inflammation of the meninges of the brain or spinal cord is almost entirely absent, or limited to a small area. We are, therefore, justified in concluding that cerebral symptoms, occurring in acute rheumatism along with hyperpyrexia, point rather to functional perversion of the brain than to structural changes in it or in the

[a] Ziemssen's Cyclop. Med. Vol. XVI., p. 43.

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heart or pericardium, and that the high temperature and the cerebral symptoms are both alike due to one and the same cause, affecting the psychical and motorial functions of the brain in the one instance, and the centres (supposing such to exist) presiding over the production of heat in the other.

This leads me to remark how little we really know of the cause of fever heat, or the mechanism by which it is produced. It is not surprising that our explanations of these bursts of high temperature in rheumatic fever must be unsatisfactory and entirely theoretical, when even the causation of the regulation of the temperature of the body in health is a matter upon which diversity of opinion exists among the most distinguished physiologists. For instance, Senator, following on the tenets of Virchow's school, maintains that "the temperature of the body is a direct consequence of the chemical processes of life." Then Dr. Burdon-Sanderson[a] states that "at all times in the normal body the available means of heat-production is in excess of the demand," and supposes that any rise in temperature is at once automatically kept in check by the nervous system.

H. C. Wood[b] arrived at the conclusion, from numerous experiments on dogs, "that there are calorific nerves, and that a rise of temperature and heat-production, which follows separation of the pons from the medulla, is paralytic and due to the removal of some active force."

In a recent paper Dr. W. Hale White[c] adduces a series of cases of nervous diseases accompanied by variations of temperature, which bear out the results arrived at by physiologists by experiments on animals, and which point, from a clinical point of view, to the existence of a heat centre situated on the cerebral surface, adjoining the fissure of Rolando, as indicated by the experiments of Wood and Hitzig. He records cases of elevation of temperature to 107·4° in tumours of the brain, in the pons and its neighbour-

[a] The Production of Heat in Fever. By W. Withers Moore, M.D. Brit. Med. Journ. 1884. Vol. I., p. 258.

[b] Guy's Hospital Reports, Vol. XLII., p. 75.

[c] Ibid, p. 49.

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hood; to 109·2° (Gowers) and to 110° (Bastian) in hæmorrhage into the same locality; to 108° in softening of the pons (Mills); and to temperature varying from 106° (Churchill) to 110°–111° (Weber) in cases of fracture and injury of the spine. His conclusions are, "that these centres in the cerebral cortex are perpetually exercising a restraining influence in the temperature of the body, and that from them fibres go to the thermogenetic tissues of the body, passing near the central ganglia down the crura cerebri through the pons, decussating somewhere below that, and going by means of the medulla and cord to the tissues."

However plausible this theory may be, it fails to satisfy inquiry, and granted even that it holds good in cases of purely non-febrile diseases, will it help to explain the occurrence of fever-heat? Is this loss of nervous control the essence of fever? To these queries a negative reply must be given. Although this theory is advocated by Dr. Handfield Jones,[a] Dr. W. Withers Moore[b] temperately discusses this neuro-paralytic theory of fever-heat and discards it:—"The theory, then, that the liberation of heat at the surface of the body is controlled or restrained in fever, through disorders of the nervous centres, so that by 'retention' the temperature rises, must for the present be abandoned, until more reliable evidence can be adduced in its favour, and we are of necessity compelled to look upon fever as a disorder of protoplasm."

But even here difficulties beset our path, and no satisfactory solution of the problem is forthcoming. For, if the development of heat be owing to excessive combustion of protoplasm, whether due or not to the arrest of the inhibitory or check action of the nervous centre, "there ought to be a proportionate *débris* of combustion manifest somewhere if," as Dr. Moore puts it, "the fire have gone on burning in excess of the needs of the body from neglect of the regulator." And yet there is not found in many cases corresponding increase in the solids of the urine, or of the other excreta.

From what I have shortly stated it will be readily acknowledged

[a] Med. Times and Gaz. 1877. Vol. I., p. 599.
[b] Loc. cit.

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that the subject is one of great difficulties, and that we must be content to observe and record such facts as come under our notice as clinical physicians, and await their elucidation in some future time at the hands of physiologists.

But few words are necessary on the subject of treatment in these cases of rheumatism complicated by hyperpyrexia, with or without nervous disturbance; not because the subject is one of little moment, but because the general consensus of opinion is in the direction of reducing the excessive heat in as short a time, and in as efficient a manner, as possible, although there may, however, be a difference of opinion as to the best method of attaining this end.

Antipyretics may act directly or indirectly, and, for the sake of description, may be classified into those which reduce the temperature in accordance with physical or physiological laws.

To the first class belong the direct and rapid cooling of the body by abstraction of its heat by contact, through the skin and mucous membranes, with surroundings of a lower temperature. To the latter class we refer medicinal remedies (quinine, Werburg's tincture, salicin and the salicylic compounds, aconite, and the ordinary diaphoretic and diuretic drugs of every-day use)—a long list, to which kairin has been recently added.

I have had no experience of kairin, but from what I have read of its effects I shall not expect very much from its employment.

I am aware of the very high opinion in which some practitioners hold salicin or the salicylic compounds as antipyretics in acute rheumatism, but I cannot unqualifyingly join their ranks. I have employed this class of drugs, and particularly salicin, as my routine treatment in that disease for many years, and in the very great majority of cases relief to pain and lowering of fever have followed its employment for a few hours or days; but, on the other hand, I have failed, in some instances, either to reduce temperature or to relieve pain by it when given in full doses.

An instance of this is at present under observation. A patient is in Sir Patrick Dun's Hospital in the second week of acute articular rheumatism. She suffered and complained of much

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arthritic pain, particularly in the ankles and knees, which are swollen. Temperature, 102°. Salicin in 20 grs. every third hour gave no relief; then in 30 grs. every hour for four doses, with a like result; and, lastly, 30 grs. every hour for six doses gave *no* relief from the pain, and reduced the temperature by but one degree. A second case occurred the following week, in which 40 grs. of salicin were given every hour for six doses, and then every fourth hour, without the least benefit to the pains or reduction of temperature. This I considered a fair trial, for, although Dr. Quinlan[a] has stated that he has given salicin without injury or any apparent effect to healthy individuals in doses of 120 grains, and that he has successfully treated rheumatic fever with doses of 50 to 60 grains every hour, I felt that my limited experience of the drug did not justify me in pressing the dose after reaching the limits laid down by Dr. Maclagan,[b] to whom the profession is indebted for this valuable anti-rheumatic, and whose words are worth recalling. He says:—"What I recommend is, that from 20 to 40 grains should be given every hour for six hours, or until pain is relieved (which it generally is within that time), and that the same dose should then be given every two hours till the pain is gone, and the temperature at or near normal (which is generally the case within twenty-four hours). After that the same dose is given at widening intervals of three, four, and six hours for ten or twelve days."

The effect of quinine on the hyperpyrexia of acute rheumatism is but slight, if indeed it will in any way check a temperature which is rising. It has quite failed to do good in my experience, even where, in one case, I employed four hourly doses of 5 grains each, and then one full dose of 20 grains. Indeed, from the analogy of its use in intermittent fever, no good can be expected of it if given while the fever is rising, however useful it is as a specific when given in the apyrexial intermission of that disease, or as a great aid to lowering the temperature which has commenced to fall.

[a] Brit. Med. Jour. Dec. 6th, 1884. P. 1124.
[b] Brit. Med. Jour. 1883. Vol. I., p. 713.

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The prompt and early application of cold to the surface is, therefore, the most valuable mode of treatment of the hyperpyrexia in acute rheumatism, as it is in the similar condition of insolation or heat apoplexy. The chances of its efficacy are greater the earlier it is had recourse to. The temperature cannot safely be allowed to rise above 105°.

Of the various methods of thus cooling the body by direct abstraction of its heat, the cold bath, cold affusions, irrigation by Leiter's tubes, and the application of iced cold cloths or sheets, are open to selection. In private practice, and where numerous assistants, necessary for the immersion of an adult in acute rheumatism in a bath, cannot be had, I have no hesitation in saying that the latter method (that by cold towels applied as I have described) is the one best suited to meet the requirements of the case. It is easily done, requires no skill, and is free from the objection and the risk of frequent handling and lifting of the patient, which is inseparable from the use of the cold bath; the strength of the patient is thereby economised, and by its readiness of application the exact amount of good required can be more accurately gauged than by immersion in the cold bath.

While thus I advocate the employment of this severe remedy to meet an extreme necessity, I must not be understood to urge it as an antidote to the disease which called for it, and I by no means ignore the depressing effect it has upon the patient's strength. I desire, therefore, to lay special stress upon the right importance which should be accorded to this treatment. It may be the means of saving life, but it is only an adjunct to the general treatment of the patient. It is directed to meet but one complication of the disease, and it is a weapon of offence we must be ready to lay aside and take again as the necessity for it may arise.

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A CASE OF SENILE DEMENTIA, WITH SOME REMARKS ON THE TREATMENT OF THE AFFECTION.

By HENRY KENNEDY, A.B., M.B.;
Physician to Simpson's Hospital, and the Whitworth Hospital, Drumcondra.

[Read in the Medical Section, Friday, March 27, 1885.]

The following case I have thought worthy of record, as it seems to me to present some features not unworthy the notice of the Medical Section of the Academy. Probably any remarks will come in better after the case has been detailed:—

Case.—An old lady, above eighty years of age, had the neck of her thigh-bone broken by a large dog, in his gambols, jumping up on her. The accident occurred in the West of Ireland, and fourteen days subsequently she was, with considerable difficulty, brought up to her home, at Kingstown, where she came under the care of Dr. Beatty, of that town. The accident occurred in September, 1884. Nothing particular was observed for a period of six weeks, when she began to get very restless and entirely sleepless, and she never ceased muttering to herself, nor did she recognise any of her family round her. This state went on day and night, though, during this period, many remedies were used, such as her condition seemed to require; still there was no amendment. Hitherto I had been in correspondence with Dr. Beatty, but I now saw her with him, and it was agreed that she should be put upon the use of the extract of hyoscyamus, of which she got three grains three times a day. At the end of a fortnight there was little change in her state, but her family thought that, if anything, she was quieter. It was now determined that a quarter of a grain of nitrate of silver should be added to each pill, but neither did this change produce any more amendment, and, finally, we agreed to give her mercury in small doses, still combined

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with the hyoscyamus. Under this plan it was pleasant to observe the slow, but steady, progress she made towards recovery. At the end of a fortnight she was, in a marked degree, better, and, when a month had elapsed, we were able to give up the mercury, as both her mind had returned to its natural state, and the terrible restlessness subsided. Her sleep, however, had not come round, and, according to the account of her family, she literally did not sleep night or day. Under these circumstances a plan was put in force which happily succeeded in causing sleep. It consisted simply in placing the patient with her head to the north. On this latter point I will speak again, and at the present moment shall only add that both her mental state and sleep seem to be completely restored, and Dr. Beatty can bear out this statement to the fullest.

The details of this case have been purposely compressed into the smallest space. It will be necessary, however, to bear in mind that it occupied seven months, during most of which period the strain thrown on her family was most trying, whilst the health of the patient herself was seriouly shaken. Besides the duration of the attack, it is also worthy of note that the patient took her food all through, and whatever was given her was swallowed automatically, as it were.

There are few who have been in practice any length of time who have not met cases similar to the one detailed—in fact, they constitute a very important class, and many of the aged suffer from the state. All the standard works on mental diseases contain accounts of it, and it, of course, presents, like other affections, an endless variety in degree. Our asylums all contain instances of the disease. Restlessness is its great and leading feature, and I have known different persons, in easy circumstances, affected with this state alone during the last few years of their lives. They were quite able to care for themselves, at the same time that they were affected with the constant desire to go about, sometimes through the city, and again taking even journeys on the railways without any ostensible object. Independently of what is known as ordinary bodily exercise, the restlessness may show itself in many forms, and when an individual in advanced life takes to the felling of trees,

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the question at once arises, is such a person affected with it?[a] But though this restlessness may exist *per se* it is more commonly united with a disturbed mental state, and to this again may be added sleeplessness, as occurred in the case with which I commenced these remarks.

And this leads me on to the question of treatment, as to how far relief can be given to these trying cases. I recollect the period—not so very long ago—when it was not thought advisable to make any special effort to afford relief—at least it was not attempted; and beyond watching the patient nothing else was done. But science has given us new powers, and the introduction of chloral has afforded us most valuable help, in some at least of the class of cases of which I have been speaking. It has some drawbacks, however, and I believe there are other drugs which are more generally useful—of these I would name hyoscyamus. I need scarcely say that this was an old remedy for mental affections, and why it should have fallen into disuse it is not easy to say. It is quite true that at the present day it is still used for puerperal mania. The late Sir Dominic Corrigan used it freely in the sleeplessness of fever; and, more recently still, its alkaloid has been used in some of the English asylums, and with truly marvellous results, such as where the patients were most violent and destructive, and utterly sleepless. In these cases it was used subcutaneously. But, even keeping these facts in mind, it does seem to me that this drug is in less general use than it might be, and particularly in the form of mental affection to which your attention has been drawn. For several years I have myself used it, and in the form of the simple extract. It is in this form it can be most easily and effectively given; but if we want to attain its full effects it must be used at regular intervals in full doses, and continuously. Some think that a dose at bedtime will be enough, and when this fails it is given up. But this is a mistake. Let it be given three times or oftener through the day, and if benefit is to come it will soon

[a] I know of an individual at the present time who is very far advanced in life, and who has taken to climbing into trees, causing much anxiety to his friends.

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declare itself; for, like all other medicines, this one, too, may fail, and in the very case before us its effects were not as striking as in other similar instances I had seen. It was the distinct opinion of the family, however, that from the time the medicine was begun the restlessness of the patient had somewhat improved. Still the change was not of that decided character which satisfied either Dr. Beatty or myself, or would justify us in simply continuing the hyoscyamus. It was then the idea occurred that possibly mercury, cautiously given, might be of use, and under this impression grey powder, to the amount of two grains each day, was administered, joined at the same time with the extract of hyoscyamus, which she had been already taking in doses of three grains three times a day. Under this plan it was really interesting to observe the improvement which began within the next eight days, and went on continuously till her ordinary mental state was quite restored.

But it will be asked here—Why venture on such a remedy as mercury, and in such a case? I can only answer that all the remedies known to us had got a full trial, and combined in a variety of ways. Chloral did not suit her at all—in fact, everything had failed to do good. In the next place, I had been long in the habit of using mercury in small doses in those cases where cerebral symptoms were present in connexion with the fatty diathesis, and the result had been most striking and satisfactory. I speak of such symptoms as giddiness, slowness of speech, tendency to stupor, forgetfulness, and a tendency to paralysis, rather than the disease itself. When such are present I repeat that mercury frequently acts like a charm, and I have used it sufficiently often to enable me to speak with confidence on the point.

In the last place, I was familiar with the changes which go on, almost as a matter of course, in those above seventy. The brain, we know, begins to atrophy, whilst its place is supplied by serous effusion both on the surface and into the substance of the organ itself. And here I cannot but question the views of some of the London physicians, and of Dr. Wilks in particular, who state that these serous effusions are of no consequence whatever, and do not affect the health of the individuals either mentally or physically. I cannot accept such a view, and, on my part, believe that the

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state is essentially one of disease, and gives rise to symptoms which are quite tangible. Besides, I have seen several instances where a sudden attack of apoplexy ended life, and when these were examined *post mortem* nothing but a large serous effusion with a wet brain was found. There was no rupture of any blood-vessel, and, in point of fact, the patients had died from serous apoplexy, described long since by Abercrombie. In addition to these points, I may again allude to the very marked effects of the small doses of mercury already spoken of, and which certainly have, in most cases, a control over those cerebral symptoms already described. The effect in many cases, it is true, is but temporary; but the medicine may be used again and again, and with the same beneficial results. It was thoughts like these which induced me to suggest mercury in the case detailed, and with such satisfactory effects.

In the last place, I would ask attention to the measure adopted with the patient to induce sleep, which had not returned with the improvement in the other symptoms. I had read that the position we occupy in bed had a material influence on our sleep, and that the best position was with the head to the north; and when I became attached to the Cork-street Fever Hospital, I was determined to test the plan, and certainly the results were quite sufficient to satisfy me that a quieting influence was exercised by the measure. It did not affect the majority of the patients, but there was quite enough to prove to me that it was a plan which should at least be tried. It seemed to me to influence children more than adults, and I cannot doubt that it is more potent amongst the middle and upper ranks than amongst the labouring classes. We know, indeed, that the former, when attacked with fever, are very much more likely to suffer from sleeplessness than the latter, and that then the symptom may become a most formidable one, affecting life itself. More could be said on the point, but these remarks must close; and it only remains to state that, in the case brought before you this evening, the patient was directed to have the position of her bed altered, and, strange to say, she slept well the very first night, and has continued to do so for the last month, though before that time her nights were entirely sleepless. It will be allowed, I think, that the plan proved signally successful.

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ON THE FORM OF PNEUMONIA PREVALENT IN DUBLIN.

By JAMES LITTLE, M.D., F.K.Q.C.P.;
Physician to the Adelaide Hospital, Dublin.

[Read in the Medical Section, May 1, 1885.]

During last winter and spring in Dublin, pneumonia, or pleuro-pneumonia, has been very frequent and exceptionally fatal. I have arrived at this conclusion not only from my own experience but from what I have been told by professional friends. Indeed, the announcement one often sees of "death from inflammation of the lungs" shows that pneumonia is extensively prevalent and exceptionally fatal. Having had a very rapidly fatal case just before our last meeting, I asked Dr. Montgomery to put my name down for this communication in the hope that, from the many members of the Academy who have had extensive opportunities of seeing the disease, we should get valuable information as to the ætiology, clinical history, and, more particularly, the treatment of the disease. I propose to give a brief account of the cases which have fallen under my own notice during this winter and spring. In my hospital beds there have only been two slight cases of pneumonia, and all the cases to which I shall have occasion to refer occurred in private practice:—

Case I.—On November 9, 1884, I saw, with Dr. Samuel Mason, a lady, aged about thirty. The disease affected both lobes of the left lung. The physical signs were dulness on percussion, and tubular breathing. There was also extensive general catarrh. Dr. Mason had treated her with quinine, and she recovered.

Case II.—On Nov. 29 I was asked to see a very delicate boy, aged eleven years. Three weeks before coming under my care he arrived from the country with his father, occupying apartments in

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Great Brunswick-street. After a few days of ordinary catarrh, the boy was attacked with severe pain of the left side. I found dulness on percussion posteriorly; tubular respiration; typical pneumonic sputum; and a temperature of 104°. There was a very extensive crop of herpes of a virulent kind. The crop on the face was on the opposite side to that on which the pneumonia existed. There was a crop of herpes on the shoulder, and violent conjunctivitis. He was treated with a hypodermic injection of morphia, and moderate doses of quinine in effervescence. The case terminated favourably about the tenth day.

Case III.—On Dec. 23rd I saw, with Dr. John W. Moore (to whom I am indebted for the notes of the case) and Dr. Harley, a gentleman, aged about thirty-five. On Dec. 20th he had become chilled while watching a foot-ball match. On Dec. 21st he had a sudden rigor while at dinner. On the 22nd Dr. Harley found the physical signs of pneumonia of the right upper lobe. During the following nine days the patient had great cerebral and respiratory oppression, with the indication of over-filling of the right side of the heart and general bronchial catarrh. On the evening of Dec. 26th the pulse was 140, the respiration 60, and the temperature 104°. On 1st January he was attacked apparently with acute peritonitis on the right side of the abdomen, with great constipation. He was relieved by leeching. One of the striking features of the case was that most of the symptoms seemed to arise from congestion of internal organs independent of the lung. He had stupor from apparent congestion of the brain; and the only treatment conspicuously useful in averting the tendency to death was free leeching. He was a very full man, who lived freely and had led an indolent life. He recovered from the pneumonia, but with damaged kidneys.

Case IV.—On Thursday, 1st January, 1885, I saw a lady with Dr. Browne. On the previous Sunday she got a chill, and on Tuesday she was found by a friend sitting over the fire, apparently very ill. She was got to bed, and being some months pregnant, she next day had a miscarriage. On Thursday there was evidence of the lower lobe of the right lung being affected, and she was in a state of great prostration. She begged to be let alone, and said she was dying and in great pain. Beyond a hypodermic injection of morphia to relieve the pain, and the use of some stimulants, nothing could be done, and she died that night.

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Case V.—On January 9th I saw a gentleman, aged fifty-five, a healthy, temperate man, except that he had lived for several years in the tropics, and was supposed to have injured his heart by excessive smoking. He had suffered from an ordinary catarrh. Having made a journey fifty miles from Dublin and back, he dined at his Club, but felt so ill he went to bed. Next day he was seen by Dr. Franks. On the following morning I saw him. He had signs of general bronchial catarrh over the whole right lung; dulness on percussion; tubular breathing, and intense pain over the lower part of the left chest. He had abundance of typical pneumonic sputum. There was more pure blood than one ordinarily sees. The striking symptom was the irritability of his stomach—constant vomiting. He was entirely sleepless. On the fourth or fifth day of the illness he was jaundiced, and there was evidence of involvement of the lower lobe of the right lung. He died on the seventh or eighth day, not having had any sleep during the whole of the illness, and he was perfectly conscious. The treatment was free dry cupping of the chest, an effort to obtain sleep being made with the bromides, effervescing draughts and digitalis pill. Dr. Franks and I discussed the use of quinine. We determined not to give it to him—first, because we thought it would not suit in presence of the want of sleep; and next, the gentleman told us that when in China he had been given quinine on account of malarial fever, and it did not agree with him.

Case VI.—On January 14th I saw, with Dr. More Madden, a precisely similar case, that of a gentleman, thirty-five years of age. He had catarrh, then pneumonia of the lower lobe of the left lung. The temperature, which had been higher, had, when I saw him, fallen to 102°. There was intense typical tubular breathing over the affected lung, dulness on percussion, and pneumonic sputum. The treatment which Dr. More Madden had adopted consisted of free blistering of the chest, with the use of blue pill, squill and quinine in tolerably large doses. He had begun to improve when I saw him. There was no alteration in the treatment, and he ultimately recovered.

Case VII.—On Feb. 7th, I saw a case with Dr. Corley, which differed from the others—that of a delicate lady, thirty years of age, who had on previous occasions been under my own care, for irritative dyspepsia. She had been ailing for some days, but her friends did not attach much importance to her illness, because of her

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delicacy. She had purged herself violently. When Dr. Corley saw her he found the physical signs of pneumonia of the lower lobe of the left lung. There was an extensive pericardial friction audible on the day of our first consultation. Even then, though she was only ill four or five days, there was evidence of nervous disturbance. She spoke of her condition with a degree of lightness unnatural in her illness and sober temperament. From that she became difficult to manage, would not take food, and became sleepless. She died in consequence of the disturbance of the nervous system. There was typical pneumonia of the lower lobe of the left lung, with pericarditis. In that case the treatment was directed mainly to the support of the patient and the relief of pain by hypodermic injections and the application of leeches.

Case VIII.—On Feb. 4th I saw, with Dr. Kidd, Dr. Henry Hadden, and Dr. David Hadden (to whose kindness I am indebted for the notes of the case) a lady, aged twenty-five. This lady, who was seven months pregnant, had been suffering during the last week of January from general bronchial catarrh. On the 30th she had a rigor and severe vomiting, and during the next few hours the temperature rose to 104·6°. On the morning of Feb. 1st the temperature was normal, and, with the exception of a few bronchial râles, there were no signs of illness; but in the evening she was suddenly attacked with severe stitch in the right side, and the temperature rose to 104°. On the 2nd there was dulness and tubular breathing over the lower right lobe, the face was dusky, and she was delivered prematurely. The treatment had been dry cupping and quinine in effervescence. On Feb. 4, when I first saw the lady, she had urgent fits of coughing, which caused intense pain, and during these she became violently excited and alarmingly short of breath. The sputum was scanty and viscid, and the soft palate covered by a thin membranous film. On Feb. 6 she complained of pain in the lower part of the left side of the chest, and a pleural friction was audible in this situation. On the seventh day of the illness resolution appeared to have commenced in the right lung, dulness decreased, tubular breathing gradually disappeared, and crepitus redux became established. The temperature, however, remained constant at about 101°, the pulse varied from 96 to 110, and the respirations at about 28 per minute. The cough was kept fairly under control by $\frac{1}{12}$ gr. of morphia under the skin each morning, and $\frac{1}{6}$th gr. at night, but occasionally produced great

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distress, especially late in the afternoon, when it was accompanied with intense nervous excitement, during which she would fling herself about, force her way out of the bed, and become completely unmanageable. These paroxysms were always ushered in by the severe fits of coughing, and were attributed by herself to "want of breath." They invariably ceased on getting the hypodermic injection. Except that the lochiæ were scanty, there appeared to be no uterine complication. There was some diarrhœa. On the evening of February the 15th the excitement became more severe than on any previous occasion, after which she became completely exhausted. She talked incoherently; her pulse became so feeble and rapid that it could not be counted, and death appeared imminent. Under the influence of stimulating enemata reaction set in, during which the temperature rose to 106°, and, except when asleep, there was constant low muttering delirium. She died on the evening of February 17th, the eighteenth day of her illness. It should be noted that this lady's husband lay at the same time, and in the same house, seriously ill with pneumonia of the right lung, from which, however, he recovered.

Case IX.—On Monday, March 2nd, I saw in Harcourt-street a lady, aged about sixty. On the evening of March 1st she was in a very crowded church, and having been seized with palpitation, to which she was subject, she left and waited in the porch until her maid got a cab. She felt chilled, but all right when she got home. Early on Monday morning I got a note saying that her pain was so intense that she felt she would die if not soon relieved. On my arrival I found the skin very hot, the face flushed, and the pulse full and strong. Immediate relief followed the injection of a sixth of a grain of morphia, and the application of a few broad and long strips of rubber adhesive plaster so as to restrain the movements of the chest, and I prescribed a minim of tincture of aconite every hour. Next morning, although there were not the physical signs of pneumonia, there was distinct pneumonic sputum. On Wednesday she was better, and insisted on giving up her medicine. She was bringing up abundant pneumonic sputum. On Thursday morning she was much worse, and I had the advantage of the advice of Dr. Head, who usually attended this lady, and who then took charge of the case. The pain was intense. There was pneumonic sputum; but beyond slight dulness on percussion and an approach to tubular breathing, there was no physical sign of

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pneumonia. There was distinct pleural friction sound. Next day there was intense tenderness of the abdomen below the right costal arch, and she became delirious, and died on Friday morning.

Case X.—On March 10th I saw a gentleman, aged about fifty-five, with Dr. Henry Kennedy. He had been exposed while attending a fair in the country, and had been ill some days before he sought advice. Dr. Kennedy found pneumonia of the lower lobe of the left lung. At the period of our consultation the pulse and temperature had fallen, and I ventured to take a favourable view of the case. Dr. Kennedy, however, thought more gravely and, as the result proved, more correctly of the case, as the inflammation subsequently attacked the right lower lobe, and death ensued. The treatment consisted of blisters, abundant nourishment with stimulants, which the patient's previous habits rendered necessary, and the administration of expectorants, with small doses of mercury.

Case XI.—March 14th, Mr. O'Grady and I saw a young lady who had often been under my care, but was at the time his patient on account of an exaggerated spinal curvature, the inconvenience of which he had, by skilfully applied support, almost entirely relieved. She was then seized with pneumonia of the lower lobe of the right lung. There was dulness on percussion, and the tubular breathing was exceedingly well-marked. The treatment consisted in relieving her pain by poultices and anodyne applications, and assuaging her cough. She was, however, a very unfavourable patient, and died in a few days.

Case XII.—On Saturday, March 21st, I saw another case in the house in Great Brunswick-street in which had occurred the second case I have narrated. This patient was the proprietor of the house, a man aged thirty-five, and who had been a patient of mine for bronchial attacks. He was a large, full man, who, though temperate for some months, had formerly considerably abused the use of stimulants. On the previous Thursday he had been at a funeral and got chilled in the mortuary chapel. On Friday night he walked home late. He did not feel chill, but he remained sleepless all night, and early in the morning complained of pain in the right side. Dr. Wallace Beatty saw him early, and made the diagnosis of pneumonia. Besides the signs of pneumonia of the lower lobe of the right lung, there was general bronchial

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catarrh, typical pneumonic sputum, diarrhœa, sleeplessness, delirium, and towards the end rapid failure of the force of the heart. Iodide of potassium had often given this patient signal relief in catarrhal attacks, and I therefore used it with digitalis. He was also freely dry-cupped, and towards the close had turpentine and brandy. Death occurred on the sixth day. Dr. M'Dowel also saw this patient.

Case XIII.—On April 3rd I saw a young gentleman at Dalkey, with Dr. Newland. The illness came on abruptly, and apparently in consequence of a chill while walking home in the evening. He was treated with large doses of quinine, which Dr. Newland gave to the extent of forty grains in twenty-four hours, for the first two days, and afterwards in smaller doses. The inflammation affected both lobes of the left lung. On the fifth day there was violent delirium, and general pyrexia persisted for three weeks, but the patient ultimately recovered.

Of these thirteen cases, eight died. The causes of death varied; it arose in some from the amount of lung tissue affected, and the consequent impediment to respiration and circulation; in others from the failure of the heart's action; in others from the spreading of the inflammation to the adjoining serous membranes; and in some the chief danger appeared to depend on the congestion of distant organs, as the brain and the kidneys. In nearly every case there was a distinct history of chill, which especially occurred after leaving a crowded building and going into the open air. It is open to those who advocate the doctrine that pneumonia is a truly infective disease, to say that the air in an overcrowded building would be specially favourable to the spread of an infective disease, and that the chill played only a subordinate part in starting it. In two instances two persons had pneumonia in the same house. In two cases an abortive crisis occurred. The frequency of pleural inflammation is worthy of note. According to my observation, the most fatal symptom in pneumonia is severe pain, and more particularly tenderness on pressure. I am anxious that the discussion should turn chiefly on the treatment of the disease. Dr. Wilson Fox, in his paper on pneumonia in Dr. Russell

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Reynolds' "System of Medicine," speaks of Dr. Hughes Bennett having selected pneumonia as a disease on which to experiment, with a view to ascertain whether inflammation could be most effectually controlled by depletory or stimulating measures, or by being left alone; but Dr. Fox observes there is no disease less fitted for such an experiment; because, while there are some cases of pneumonia certain to get well on any treatment, there are others almost certain to die. I am of the opinion which has been frequently expressed by Sir Dominic Corrigan—that the physician should always remember that he is treating not the disease but the patient; and while I am sure that in every case of pneumonia, as in all others, the physician must take into consideration the peculiarities of the patient and the details, still in such a disease we may, I think, arrive at some conclusions, at least as to what remedies are useless, especially with regard to a particular epidemic, even though our conclusions may not be applicable to other outbreaks of the disease. As far as I have been able to form an opinion, I will mention the remedies which appeared to me to exercise a distinct influence over the disease. Where it begins with a chill and rapid elevation of the temperature, the use of aconite in frequently-repeated small doses (say a minim of the pharmacopœal tincture every hour, for eight or ten hours, and then less frequently) gives relief, and sometimes seems to cause the disease to pursue a favourable course. When there is severe pain, a hypodermic injection of morphia, and poultices applied to the side, usually give relief. The indication for any kind of depletion, for the application of leeches or wet cupping of the chest, is overfulness of the right side of the heart. I have seen some cases in which conspicuous relief was thus given. About a year and a half ago, being accidentally near Glendalough, I was asked by Dr. Burgess to see a young man with double pneumonia, and apparently dying. He was in great distress from oppression of breathing, his pulse was small, and his face dusky. It appeared to Dr. Burgess and me that the chief cause of his danger lay in the over-distension of the right cavities of the heart, of which the signs were present. Being many miles from any place where

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leeches or cupping instruments could be obtained, Dr. Burgess applied a tumbler exhausted by the flame of turpentine over the lower sternal region and ensiform cartilage, and freely scarified the resulting elevation with a lancet. The bleeding was freely encouraged, conspicuous relief occurred, and the patient recovered. Dry cupping appears to be useful where, with consolidation of a single lobe of a lung, there is also extensive bronchial catarrh. With regard to the utility of quinine, I speak with hesitation. For some years I have generally used it in pneumonia, and it appeared to me most useful when administered in effervescence, from three to five grains of the sulphate being dissolved by the aid of twenty grains of citric acid in an ounce of water, with syrup of orange, and given after the addition of sixteen grains of bicarbonate of soda or potass, in a similar quantity of fluid. There are physicians of experience, however, who consider that quinine exercises no favourable influence over the disease, but, on the contrary, is rather disposed to check expectoration and produce danger; and the chief object of this communication will be attained if it elicits the opinions of the members present on this particular point.

I hope I shall be permitted to add that since I made to the Academy the communication of which the foregoing is the reporter's notes, I have seen four severe cases of pneumonia, in all of which quinine appeared to be useful. No one is more sensible than I am of the difficulty in judging of the effect of a remedy in an acute and apparently self-limited disease such as pneumonia; but I think the following may be looked upon as fulfilling the conditions of a reliable experiment:—About six weeks ago the wife of a solicitor in this city, a strong, healthy woman, aged nearly forty, but who had been a good deal run down by the bearing and the caring of a large family, after a few days of ordinary catarrh was seized with severe pain in the right side of the chest. She then sent for Dr. Cahill, who found a rough friction sound at the seat of pain. The next day there was pneumonic sputum and harassing cough. I saw her with Dr. Cahill on the fourth day, when there was dulness on percussion,

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and tubular breathing over the lower lobe of the right lung, with typical sputum, the general symptoms and the pulse, temperature, and respiration indicating a rather grave case; and we agreed to continue the treatment on which she had been from the commencement—namely, the administration every fourth hour of three grains of quinine in pill, with the occasional use of a cough mixture. On the seventh day, when we met again, the patient's condition was as follows:—She had passed a restless and almost sleepless night, and had been wandering more or less for the previous twenty-four hours. She could give no assistance in turning herself in bed, owing to prostration. Her skin was moist. The respiration was 50, the pulse 90, and small and weak, and the temperature 102·4.° She had a good deal of cough, and had brought up a considerable quantity of prune juice sputum. We had hesitation in raising her up, but on doing so found the evidences of pneumonia over the entire right back, and some dulness and bronchial breathing over the lower lobe of the left lung. We agreed to continue the beef-tea and brandy, which she had been taking, and to give every eighth hour ten grains of quinine in milk. On the following morning Dr. Cahill found the patient so much better that he directed the dose to be given only twice in the twenty-four hours, and this was continued until we met on the evening of the tenth day, when there was such an improvement that we agreed to discontinue medicine. In two cases in which quinine had been freely given, violent delirium occurred. Perhaps it was not due to the drug, but I rather think it was, and the circumstance suggests caution in the use of the remedy. In the last of these cases the delirium soon ceased on the substitution for the quinine of bromide of sodium in full doses, after the third dose of which sleep was obtained by the administration of fifteen grains of chloral.

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NOTES OF VISITS TO CONTREXÉVILLE AND ROYAT-LES-BAINS.

By F. R. CRUISE, M.D., Univ. Dubl.;
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[Read in the Medical Section, May 29, 1885.]

It is well known to all physicians of experience, that one of the most powerful agents we possess in the treatment of many chronic ailments, is the use of mineral waters, especially when taken at their sources. However, if it be needful to prescribe the most efficacious drugs for each case we meet, it is no less important to select for each of our patients the mineral water best suited for his disease.

To do this requires a somewhat special training and experience not easily attainable. It is most important, both for patient and physician, that no mistake should be made in the choice, as a visit to a thermal station is not infrequently a very laborious and a very expensive undertaking.

For these reasons I venture to make the present communication to the Academy of Medicine in Ireland, in the hope of giving to others some information, derived from personal experience, of two celebrated French watering-places—namely, Contrexéville and Royat-les-Bains.

A brief history of the circumstances which led to my visiting Contrexéville in the year 1877, may add to the interest of the subject, and therefore I will relate it.

Just fourteen years ago Sir George Porter and I attended a young lady (Miss M.) who suffered severely from chronic cystitis. The origin of the disease was obscure, but, from our knowledge of the patient's constitution, we inclined to the belief that a strumous diathesis was to blame. I regret to add that we failed to cure her.

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After some time Miss M.'s mother took her abroad, and while in Paris consulted the late Dr. Acosta about her case. Dr. Acosta recommended a visit to Contrexéville, and a course of the waters and baths there. His advice was followed, and the result was a perfect cure.

I need hardly say that this rapid cure of so obstinate a disease, by a visit to a mineral water of which we had never even heard, made a very strong impression upon Sir George Porter and on me, and we resolved to profit by the experience.

Before adverting to the many cases in which we advised a visit to Contrexéville, I may here record the fate of Miss M.

A few years subsequent to her cure of cystitis, she died suddenly. I learned that her death was preceded by very violent headaches. Although neither Sir George Porter nor I had attended Miss M. during the fatal illness, the family wished us to be present at the *post mortem* examination, which was deemed advisable on account of her sudden and unexplained death. The examination showed that the cause of death was a large effusion of blood at the base of the brain. The membranes and adjacent cerebral substance were found extensively infiltrated with tubercular deposits.

Sir George Porter and I felt anxious to ascertain the condition of the bladder, which had formerly been so much affected, and accordingly we examined it. As a result we found that organ, and all its appendages, kidneys included, *perfectly healthy*.

Shortly after our favourable experience of the effects of Contrexéville in Miss M.'s case, Mr. C., an elderly gentleman of high position in the legal world, came under Sir George Porter's and my care. His case was one of chronic cystitis, the result of stricture and prostatic disease. The dilatation of the stricture gave but little relief to the cystitis and accompanying sufferings. So we advised him to go to Contrexéville. He went there two summers consecutively. His case, as you will understand, was not a hopeful one for cure, but he derived very marked relief, and his life was evidently prolonged considerably.

The next case I shall allude to was that of a distinguished ecclesiastic, the President of one of the Provincial Colleges of

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Ireland, who suffered intensely from renal colic, caused by oxalate of lime gravel. His cure, accomplished by one visit of three weeks (in 1877), was complete and permanent.

A similar case, in the person of a well-known and much respected Dublin merchant, occurred (in 1883) within my own experience.

In the summer of 1882, Dr. James Little asked me to see a medical confrère (Dr. L.), who was quite broken down by chronic pyelitis. We decided to send him to Contrexéville, where he derived much relief, and, finding this cold, damp climate unendurable, he emigrated to Australia, whence he sends me good accounts of his health and progress.

I shall not occupy your time by relating details of cases, preferring to state, in brief, that since I have known of Contrexéville I have either sent there, or treated here with the imported waters, most of the obstinate cases of irritable bladder, or cystitis, which have come into my hands; and that all have derived benefit, and the more hopeful quite a satisfactory result. It so happens that my endoscopic studies have brought under my care a large proportion of cases of urinary disease, so that my experience has been somewhat extensive.

When first I learned the therapeutical value of Contrexéville, I sought in various directions for information concerning it, and encountered much difficulty in my research. Although so celebrated in France and on the Continent, this watering-place appeared unknown in England, and with the exception of the brief notice of it in the French Manual on Mineral Waters by Dr. Constantine James, of Paris, I could obtain no details. At last, one of the patients whom I had sent thither brought me two Essays on the subject—one by the late Dr. Victor Baud, who had practised there for a quarter of a century, and another, more brief, by Dr. Tamin-Despalles, who was then in practice at these waters. More recently I have found an Essay on the subject by Dr. Debout D'Estrées, the Government Inspector of the Station, which, on the whole, is the most satisfactory I know of.[a]

[a] This Guide Book can now be had, in English, from Messrs. Churchill, of New Burlington-street, London.

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Wishing much to see Contrexéville, and urged to do so by Sir George Porter, I took an opportunity while at leisure in Paris, in the autumn of 1877, to run down there and make a personal visit.

Taking the night mail from Paris, by the Chemin de Fer de L'Est, I reached Neufchâteau, in the Vosges district, about six o'clock in the morning. Having breakfasted I hired a voiture and drove to Contrexéville, a lovely drive of about fifteen miles, through a pretty mountainous country, not unlike our own county Wicklow.

I may observe here that now-a-days the train from Paris goes straight through to Contrexéville, making the journey shorter and easier than formerly.

On my arrival I was most hospitably received by Dr. Tamin-Despalles, who showed me all that was to be seen, and explained the uses and effects of the waters. I regret much to say that this experienced practitioner has since died, after a long attack of brain disease. His place is ably filled by several other physicians of eminence, amongst whom I may specially mention Dr. Debout D'Estrées, who has treated several patients I sent to him with judgment and success. I had the pleasure of making his personal acquaintance in the year 1883. To show how little was known of Contrexéville, except on the Continent, I may mention that Dr. Tamin-Despalles told me, in 1877, that so far as his knowledge went, and it extended over a long term of years, I was the first English-speaking physician who had ever visited it. Since that time it has come under marked notice, and has been visited by many of our Irish, English, and American confrères; however, as I have not seen any account of their visits, in our language, I think a brief note of my experiences may interest the members of the Academy of Medicine in Ireland.

I found Contrexéville a pretty little town, or rather village, made up, for the most part, by the hotels and boarding-houses which accommodate the great influx of patients, who come to it for the season, from May until about the 15th of September. In former days it belonged to the kingdom of Lorraine, but now it is in the department of the Vosges.

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Situated on the river Vair, in a valley open from north to south, it is placed at a height of over a thousand feet above the sea level. Its situation explains the sudden changes of temperature to which it is subject, especially in the morning and evening. This fact must be borne in mind by visitors, who should always be provided with woollen garments, even in the height of summer.

Though the waters of Contrexéville have been locally celebrated time out of mind, they were only made known to the medical world in 1760, by a report read before the Society of Science and Arts of Nancy, by Dr. Bagard, Physician to Stanislaus, King of Lorraine.

I shall read a short extract from this quaint Report which will show that even at that distant time Contrexéville was resorted to by patients suffering from gravel, bladder diseases, gout, and liver complaints:—

"The mineral waters of Contrexéville are a sovereign remedy in diseases of the kidneys, of the ureter, of the bladder and the urethra, stone, gravel, glair, suppuration. We can assert that these waters are efficacious against the stone, which they loosen and drive out of the bladder, when the stone is not of a considerable size; that they dissolve and break larger stones, when the latter are chalky. We keep a list of patients of all ages, who, during several years, have got rid of a stone through the effect of these waters.

"They prevent fresh attacks of gout by restoring the suppleness of the nerves, and of the membranous parts which the disease has dried up.

"As these waters contain ferruginous matter, a mineral acid and soap, they will be very useful in cases where the bile becomes too thick and where the liver is obstructed—the more so as they are purgative."

Dating from the time of Dr. Bagard's Report we find the celebrity of Contrexéville steadily growing. Analysis upon analysis has been made of the waters—the Government has taken the station under its protection. Capitalists have expended large sums in making the place attractive to visitors, and year by year the number of these visitors has increased, so that now, owing to the crowds, it

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is necessary to secure accommodation in advance before venturing to undertake the journey. The accommodation at present is quite first-rate. There are capital hotels and boarding-houses to suit various means; also a well-regulated Établissement for baths, douches, &c., a very pretty park, with suitable amusements, casino, theatre, covered promenades, music, reading-rooms, &c.

It is right to add that travellers who seek amusement alone are likely to be disappointed. The place is remote and somewhat dull, the patients who go there are not young or lively; but the cures are numerous and remarkable, and the patients generally are very well satisfied.

I was greatly struck in observing the wisdom of the French Executive, which, like all other Continental Governments, takes under its guardianship all places possessing mineral waters of value, developing their utility, and doing full justice to their virtues and capabilities—a contrast very humiliating to anyone familiar with the neglect exhibited by our Government towards places similarly gifted in Great Britain and Ireland.

Contrexéville is prettily situated in the heart of the Vosges mountains, surrounded by agreeable walks and drives, and within easy reach of many interesting places. Amongst others I may mention Domrémy, the birthplace of Joan of Arc; Mirecourt, a flourishing town, celebrated for the manufacture of musical instruments; Neufchâteau, the valleys of Viviers and Bonneval, the baths of Plombières, and similar places of interest. Trips to these various resorts pleasantly fill in the leisure hours of the patients staying at Contrexéville.

As already mentioned, I visited the different springs, and ascertained their properties.

Four principal springs are used, which I name in the order of their importance—namely:—

The Pavilion;
The Prince;
The Quay;
The Sovereign.

A fifth, named "Le Cler," is also used, and its water exported,

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but it is weak compared with the Pavilion. All these springs are cold.

The Pavilion may be taken as the typical and most important water. I give here a table, showing the result of M. Debray's analysis :—

Analysis of the Pavilion Water.

Free Carbonic Acid - - -	0·080[a]
Bicarbonate of Lime - - -	0·402
„ Magnesia - -	0·035
„ Iron - - -	0·007
„ Lithium - - -	0·004
Sulphate of Lime - - -	1·165
„ Soda - - -	0·236
„ Magnesia - - -	0·030
Silica - - - - -	0·015
Chloride of Potassium - - -	0·006
„ Sodium - - -	0·004
Fluoride of Calcium - - -	traces
Arsenic - - - - -	traces
	2·384

While we may take the Pavilion water as the most important and typical in its composition and effects, it is right to observe that the other springs are similar, on the whole, with some variations.

Thus, the Prince is richer in iron, and hence of great repute in the treatment of anæmia and diseases of females. According to Dr. Victor Baud it is specially efficacious in functional sterility.

The Quay spring contains more magnesia, and is particularly suited to the gouty.

The Sovereign is much weaker than the others, and contains no iron, or, at all events, none worth notice.

The "Le Cler" resembles the Pavilion, but is weaker. I draw attention to this fact as it is now imported, but it is by no means as efficacious as the Pavilion.

[a] Grammes per litre.

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It is very important to observe that these waters bear carriage well, the only difference observable in the analysis made at the springs, and after importation, is that the latter show a certain falling off in strength.

My friend, Dr. Tichborne, who has lately brought out an important work on mineral waters, in conjunction with Dr. Prosser James, of London, was kind enough, two years ago, to place in my hands the results of his analysis of the imported waters of Contrexéville. According to his researches the solid constituents of the imported water amount to 166·7 grains per gallon, as against 229·5 found in the water fresh from the spring. The "Le Cler" water shows only 149 grains per gallon.

It will be seen from the foregoing analysis that the spring belongs to the class of calcareo-sulphated waters; that it is alkaline, with a preponderance of salts of lime; that it contains aperient salines in the form of sulphates of soda and magnesia, also iron and arsenic, and traces of fluoride of calcium. It holds, moreover, sufficient free carbonic acid to make it bright and pleasant to the taste, when fresh from the source, thus masking its somewhat inky flavour.

Its effects, when used internally, may be anticipated. It is—

1st—Diuretic.
2nd—Laxative.
3rd—Tonic.

When taken in suitable doses it purges gently, improves the appetite, stimulates the circulation and skin, promotes the flow of bile, and *especially* it increases the urinary secretion in a very remarkable manner.

It is upon the urinary organs that the water of Contrexéville exercises the most powerful effect. It acts rapidly, producing a diuresis far in excess of the quantity of fluid ingested; it dissolves the mucus met with in the passages, and loosens and carries out small concretions.

It has been observed to subdue quickly excessive acidity of the urine, clearing it remarkably of deposits, and, strange to say, in certain cases it is found to restore alkaline urine to its normal

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acidity. This seemingly paradoxical effect is due to its efficacy in curing morbid conditions of the urinary mucous membranes, which so often lead to ammoniacal decomposition, and consequent alkalinity of the urine.

Its effects in cases of gravel and stone are remarkable. When the calculi are small, they are frequently washed out bodily by the diuresis. When large and soft, they are disintegrated and broken up to such an extent that some actual cures have been recorded; but the more common effect of Contrexéville water on large calculi is to strip them of mucous and phosphatic coatings, thus exposing the central calculus in its rough outline, and increasing the calculous symptoms.

In this way the waters act as a valuable test, quickly revealing the presence of the stone, and compelling the patient to seek surgical aid, and waste no time in hesitancy, the too-frequent result of procrastination, due to the fluctuation of symptoms.

In cystitis, whether essential or symptomatic, the waters of Contrexéville are useful beyond all controversy, often curing the former, and mostly alleviating the latter cases, even when all other remedies have failed.

I can speak with confidence on this subject, from an experience extending over fully fourteen years.

The highest Continental authorities are agreed upon the therapeutic value of Contrexéville waters in urinary diseases. As I stated before, my knowledge of their utility arose from Dr. Acosta, of Paris, sending thither a case which had long resisted other treatment, and both the late Dr. Civiale, a specialist of European fame and historical celebrity, and Dr. Durand-Fardel, another specialist on the subject of mineral waters, speak in the highest praise of them. Dr. Civiale particularly advised their use in cases of atony of the bladder, arising from long-standing stricture, cystitis, and following the operation of lithotrity.

The *essential* difference observable between the Contrexéville waters and those of other alkaline sources, such as Vichy, Vals, and so forth, is that they are *tonic*, and thus, while beneficial to the local ailments, are, at the same time, restorative of the

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general health and strength. Such a combination of virtues cannot, by any means, be affirmed regarding many other alkaline waters.

Besides their internal administration, the waters of Contrexéville are also used externally, in baths, douches, &c.

I have some doubts of their possessing any very powerful effect when applied to the skin, except perhaps as a stimulant in douches, and thus an adjuvant to their internal exhibition; all the more so, as I observed at the time of my visit that the waters, when heated, freely deposit the solid ingredients, lining the boilers with thick incrustations.

Although my principal object in reading this paper to-night is to explain the value of the waters of Contrexéville in diseases of the urinary organs, I would ask permission to notice briefly their utility in some other ailments:—

First—in gout these waters will be found most serviceable, especially in the chronic or atonic forms. This might be anticipated from their usefulness in calculous disorders, which are largely due to gout. In them we find properties at once *diuretic*, *laxative*, and *tonic*, fulfilling the most important indications for treatment. Moreover, being moderate in the degree of their mineralisation, they are much safer, especially for the weakly, than the waters of Vichy, Vals, Carlsbad, and other more powerful alkaline spas, which suit best for the plethoric. Trousseau, as also Charcot, dwells on this point in favour of Contrexéville.

Another class of cases in which these waters are found particularly useful are those in which the liver is congested and inactive from one cause or other, and the bile inspissated and tending to the deposition of concretions or gall-stones. Numbers of patients thus affected are sent by the French physicians to this station, and obtain remarkably favourable results. This, as in the case of gout, is just what we might expect from the composition and effects of the waters.

Another ailment in which Contrexéville waters are used with advantage, is nocturnal incontinency of urine in children. I can speak decisively on this point from my own experience. I believe

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that in such cases the action of the water is two-fold. In the first place, it corrects irritating conditions of the urine; and secondly, it restores tone to the organs. Whether this is the correct explanation or not, of one thing I am certain, that the use of the Pavilion water, as a diet drink, has cured a number of very obstinate cases.

Another disease in which Contrexéville water has been found very serviceable is diabetes. I do not mean to affirm that it will accomplish the cure of confirmed cases of diabetes with progressive wasting, but certainly in the group of patients who are gouty, and more or less diabetic, it materially reduces the glycosuria, and proves a useful adjunct to a regulated dietary. In this effect it closely resembles the famous American mineral water, the *Bethesda*.

To recapitulate:

The waters of Contrexéville are—

Mildly alkaline, especially from lime salts.

Saline, from sulphates of soda and magnesia.

Tonic, being ferruginous and arsenical, with traces of fluoride of calcium.

The diseases in which they prove useful are—

Chronic affections of the urinary organs, especially when complicated by gravel, renal or vesical.

Gout, especially its atonic forms, these waters being tonic, and thus markedly contrasting with many other alkaline waters.

Nocturnal enuresis in children.

Diseases of the liver, especially those complicated by gall-stones.

Diabetes, especially when associated with gout.

Let me add a word as to the use of the imported waters of Contrexéville.

I advise the Pavilion spring, and none other. Of this I give half a bottle daily as a diet drink, either with milk, wine or spirit, as otherwise indicated. Watching the effect I increase the quantity up to a whole bottle, but have not exceeded that amount.

As to the method of using these waters at the springs, I do not touch the subject. Taken fresh they are far too powerful to be used without the regular supervision of a physician familiar with

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their employment. Under such direction I believe they will be found an invaluable adjunct to the resources of the practical physician and surgeon.

Allow me, before concluding, to add some notes of a visit which I paid to Royat-les-Bains, in June, 1884, and of the circumstances which brought me there:—

One day, in the spring of last year, I happened to complain to my friend and colleague, Dr. Patrick Hayes, about the great annoyance I had suffered from eczema. This affection, presumed from many special symptoms to be of a gouty nature, had been treated in various ways, but with very partial success. Dr. Hayes told me that a patient of his, similarly affected, had been sent by a London physician to Royat-les-Bains, and had been effectually cured.

A few inquiries satisfied me that this mineral watering-place enjoyed a well-merited celebrity in the treatment of chronic forms of gout, and I felt that it was well worth my while to pay it a visit. Accordingly, to Royat I went, in June, 1884, and, to make a long story short, I was cured so completely that with a little attention to diet, and some other hygienic details, I have escaped ever since all annoyance from my troublesome ailment.

It is but truthful to add that I have kept up the use of the waters, and observed the rules of life laid down for me. Many patients who are careless about these matters blame very unjustly the remedy which they unreasonably expect will cure them despite of themselves.

Anyone who has undergone for years the indescribable worry of eczema, will understand that I naturally feel very grateful to Royat, and am anxious to make known to the members of the Academy of Medicine in Ireland some details concerning it.

Leaving Paris by the Lyons Railway Station, a journey of some nine hours brought me to Clermont-Ferrand, the wealthy capital of the Auvergne district, and in ten minutes more I arrived at the picturesque village of Royat-les-Bains, one of the most celebrated mineral stations of France. Once settled in my hotel (Chabassière's), which it is but just to say combined every comfort with moderate charges, I sought the advice of Doctor Alexandre Petit,

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whose *brochure* on Royat I had already studied. Under his guidance I commenced the course, which lasted three weeks.

The routine was much as follows:—At 7 a.m. I took a glass of the Eugenie water, hot from the spring, then a reclining bath in the same water, at a temperature of 95° F. After the bath I had another glass of the water. Then I returned to my hotel, and had coffee and strawberries. At one o'clock déjeuner à la fourchette. At 4 to 5 p.m. two glasses of the St. Mart spring, with fifteen minutes between. At 6 o'clock, dinner.

Thus I passed the days, spending the leisure intervals in exploring Royat and its environs, and in observing the use and effect of the waters. Day by day I improved in health and spirits, the eczema cleared off, and no new spots appeared. As already stated, I have remained well, now just a year.

Royat-les-Bains is a village, consisting mainly of the Établissement for the waters and baths, the hotels, boarding-houses and villas, which acccommodate the visitors in the season, from May till the end of September.

It is beautifully situated in the Auvergne mountains, about 1,500 feet above the sea level, in a valley stretching from the base of the Puy-de-Dôme to Clermont-Ferrand, and following the course of a torrent of lava, which, at some remote age, poured down from the neighbouring volcanoes, now completely extinct. The lowest portion of this valley conveys the Tiretaine, a mountain stream of some considerable size.

All that a lovely country, an agreeable bracing climate, a wealth of interest, historical, geological, and botanical, combined with health-giving waters, can offer to satisfy the visitor, will be found in plenty at Royat-les-Bains. To those who love mountain scenery, as I do, no day spent there can feel irksome or tedious.

It is right to add that it is a very quiet spot, to which the world of fashion has not yet penetrated; but its fame is of old standing, it is thoroughly appreciated by the medical faculty and people of France, and yearly becomes better known to the world at large.

To prevent confusion about names, let me observe that Royat, the original village, is situated about fifteen minutes' walk higher

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up in the valley than Royat-les-Bains, and, though most interesting to visit, is not the mineral station known as Royat.

The original village is evidently of great antiquity, resembling a miniature old Roman town. The photographs which I exhibit, many of which I took myself on the spot, show the curious old streets in which it abounds, and the castellated and battlemented church, a Romano-Byzantine edifice, dating from the twelfth century.

There are clear evidences that the ancient Romans knew Royat well, and used its hot springs, both for separate baths, and for the piscine or general swimming tank. Just below the railway bridge may be seen the remains of Roman baths and piscine, close to the St. Mart spring, which were discovered in 1843. These are some of the many ruins which prove the Roman occupation of Royat in ancient times.

The neighbourhood of this station abounds in interesting places, which serve to occupy the leisure hours of visitors to the waters.

Clermont-Ferrand, to which I have already alluded, is a little over a mile from Royat, and may be reached by omnibus in about ten minutes. Expeditions are made thither almost daily, and I know no more agreeable stroll than to wander through its beautiful Gothic cathedral, and then walk down to the Place Pascal, in which we find the statue of the great philosopher, so placed as to look to the distant towering Puy-de-Dôme, upon the heights of which he performed the experiments which led to such advance in barometric science.

Selecting a clear day, a journey of some two and a-half hours brings to the summit of the Puy, with its ruined Temple of Mercury, now replaced by an observatory, from which a magnificent view of the surrounding country stretches out on all sides.

In every direction about Royat we find beautiful mountain passes, to ride, drive, or walk; picturesque old towns and villages, and ruins of ancient castles and chateaux. Not a day of the whole course of three weeks need pass without a pleasant excursion, amid scenery lovely enough to please every eye, and monuments of the old feudal times, like Tournoël, grim and gaunt enough to make one rejoice that we do not live in such wicked and awful days of

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savagery. I shall not attempt to enumerate the various points of interest around Royat, as the local guide-books offer all needful information, and I only allude to them here to illustrate the attractiveness of the place. As a matter of course, all the usual recreations of watering-places are found at Royat—a pretty terraced park, reading-rooms, Casino, theatre, music, and so forth.

Let us now see what the waters of Royat are—what is their nature—and in what diseases they may be used with benefit.

There are four springs, which much resemble each other, though differing in certain details,[a] as follows:—

The Eugenie.
The Cæsar.
The St. Mart.
The St. Victor.

Taking the Eugenie spring, as the one both typical and most important, let us examine its constitution. I append M. Lefort's analysis:—

Analysis of Royat Water.

Eugenie Spring—(Temperature 95° F.)

Bicarbonate of Soda	1·349[b]
„ Potash	0·435
„ Lime	1·000
„ Magnesia	0·677
„ Iron	0·040
„ Manganese	traces
Sulphate of Soda	0·185
Phosphate of Soda	0·018
Arseniate of Soda	0·004
Chloride of Sodium	1·728
Iodide and Bromide of Sodium	traces
Silica	0·156
Alumina and Organic Matters	traces
Chloride of Lithium	0·037
Total Solids	5·623
Free Carbonic Acid	0·377

[a] It is important to know that these waters bear carriage remarkably well, and suffer little loss in the process.

[b] Grammes per litre.

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From the foregoing table it will be seen that this spring belongs to the class of alkaline-chlorinated waters, with the addition of salts of lithium, iron, and arsenic.

The effects of the water, when used internally, may be surmised from its composition. It is—

Stimulating to the circulation.
Diuretic.
Laxative, and
Tonic.

The diuretic effect is the most manifest, often doubling the quantity of urine excreted, and at the same time raising the specific gravity one-third or more. In its diuretic properties I observed a strong similarity to our own mild sulphur water at Lisdoonvarna, which, although quite different chemically, is famous also for the cure of gouty and cutaneous affections.

The laxative effect of the Eugenie water is mild, but unmistakable, the sulphate of soda mainly accomplishing this end.

The tonic properties, due to iron and arsenic, are very pronounced, and, together with the chloride, serve to mark the difference between Royat and the stronger and more purely alkaline springs, such as Vichy, Vals, Pongues, and others, indicating, moreover, their greater suitability for lymphatic and debilitated patients—in fact, for strumous subjects, no waters, excepting alone those of La Bourboule, which is quite near, enjoy an equal reputation. At some future time I hope to bring La Bourboule under your notice.

As already stated, the four springs of Royat, although resembling each other, differ in certain points.

Thus, we have seen the composition and effects of the Eugenie spring.

The Cæsar is the least mineralised of all these waters, but is very agreeable and piquant in flavour, and is found very useful in dyspepsia. It stimulates the mucous membrane of the stomach, improves the appetite, helps digestion, and increases the flow of bile and urine.

The St. Mart water contains a considerable quantity of chloride

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of lithium, and is particularly suited for gout and rheumatism, and all allied affections.

The St. Victor spring is the richest in iron, and is specially useful for anæmia, diseases of females, and for the weak and debilitated.

All these springs are used, according to the necessities of the cases, in three different ways—

1st. Internally.

2nd. Externally—as baths, reclining or swimming (piscine), and douches.

3rd. By inhalation, while in a state of pulverisation, or minute division.

It is easy to see that with such a repertory of potent agents as these various waters afford, a judicious physician wields immense therapeutical power.

I shall now briefly enumerate the principal diseases in which they may be used with advantage. These may be divided, for practical purposes, into three great classes:—

1st.—*All forms of gout and rheumatism*, especially the atonic phases.

It is almost needless to say that a mild alkaline water, modified by chloride of lithium and ferro-arsenical salts, is certain to benefit this class of ailments, and accordingly we find them improving from day to day at Royat; including gouty and rheumatic affections of joints, muscles, fasciæ, nerves, skin, especially eczema, gouty dyspepsia, gastralgia, asthma, and so forth.

2nd.—*Anæmic and lymphatic affections.*

The combination of chloro-alkaline waters with iron and arsenic improves the appetite and digestion, and speedily reconstructs the blood in anæmia. It is found very useful also in atonic dyspepsia, and in all the nervous troubles appertaining thereto. For insomnia, Royat enjoys a special and most favourable reputation.

3rd.—*Affections of the throat, lungs, and uterus.*

The utility of the waters of Royat in this third class of ailments is what we might expect from the strong similarity between them and the waters of Ems—Royat being, in fact, the French

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counterpart of Ems, enjoying, however, the great advantage over the latter of a climate both bracing and tonic, in place of relaxing and debilitating.

Accordingly, we find chronic affections of the larynx and pharynx, bronchial catarrh, asthma, and even the earlier stages of pulmonary phthisis, improving at Royat, and the crowds which frequent the inhaling rooms give proof of the beneficial results felt by all who resort to this form of medication.

Chronic affections of the uterus are likewise found to derive immense service from these waters.

For myself I am strongly disposed to believe that the efficacy of the waters of Royat are greatly augmented by the climate, which is, like all mountain air, revivifying in a marked degree. It is, moreover, temperate and dry, and its salubrity is proved by the abundance of fruit and flowers for which the whole district is celebrated.

I shall not dilate further upon the virtues of Royat, trusting that I have already given a sufficiently clear outline to enable my hearers to select the cases best suited for the use of its waters.

I have to thank you for the patience with which you have listened to me this evening. My earnest conviction of the great value of the waters of Contrexéville and Royat-les-Bains is the best excuse I can make for trespassing so long upon the attention of the Academy of Medicine in Ireland.

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SURGICAL SECTION.

CARCINOMA OF THE RECTUM.

By CHARLES B. BALL, F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; University Examiner in Surgery.

[Read in the Surgical Section, November 14, 1884.]

The patient whose case I am anxious to bring under the notice of the Academy this evening was kindly placed under my care by Dr. J. K. Barton. She was a tolerably healthy-looking woman, aged sixty years, and the following are brief notes of her case:—

Case.—Eleven months before admission into Sir P. Dun's Hospital she first felt a lump coming down when she was at stool, and difficulty in obtaining an evacuation, with occasional hæmorrhage.

A month later she was in a Dublin hospital, where, she stated, a pile which appeared externally was removed. From that time she remained free from bleeding and pain for six months. Towards the end of last May she suffered from flatulence and indigestion, and the bowels, which had for a long time been costive, became more so, relief being only obtained after the use of strong purgative medicines, and then with considerable straining and pain. There was some slight discharge of bloody mucus occasionally, but not to any great extent. A month later she became conscious of a growth in the rectum, which partly protruded when the bowels moved; lately this had increased much in size. The pain during defæcation was considerable, and referred to a point immediately above the symphysis pubis, and she was much troubled with pruritus.

Upon making an examination the anus appeared normal, and the sphincter was not unduly relaxed. About an inch from the anal verge on the anterior aspect of the rectum two distinct and tolerably hard tumours could be felt, evidently implicating the mucous

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membrane. By passing the finger well up, the superior limits of both could easily be made out, and below them a smaller mass was to be felt. With one finger in the rectum and the other in the vagina, it was easily determined that there was no abnormal adhesion between the two canals. The rest of the rectum, as far as it could be examined with the finger, appeared normal, and no enlarged glands could be felt in the hollow of the sacrum, nor was any evidence to be found of engagement of the liver or other abdominal viscera.

On November 1st I removed the growth by the usual method, Clover's crutch being employed to keep the patient in the lithotomy position. The anus was then stretched, and an incision carried from the margin back to the coccyx, dividing the posterior wall of the bowel to the extent of about 1½ inches. The angles of the incision were held asunder, and a good view obtained of the interior of the rectum and the origin of the tumours. An incision was next carried round the anterior two-thirds of the wall of the gut, about half an inch below the attachments of the growths, and well above the external sphincter. The wall of the intestine was now carefully dissected from the vagina, until it was evident that the healthy bowel could be felt between the finger and thumb above the highest limits of the disease. A curved incision was made with a scissors well free of the mass, and the whole removed. Hæmorrhage was not as severe as might be anticipated, only two ligatures being required. There was a little oozing from a point deep in the incision between the vagina and bowel, to which the benzoline cautery was applied, and, finally, a deep suture was passed to arrest it. No attempt was made to suture the divided portions completely. The wound was thoroughly well washed with a solution of corrosive sublimate—1–2,000—and a sanitary towel wet in the same solution was applied.

The progress of the case has been quite satisfactory. The temperature never reached 100°—indeed, for a few days it was sub-normal, during which time she was much depressed.

The bowels moved on the fourth day, and again on the eleventh; each time she had complete control of the motion, and has not since been troubled with the least incontinence, the discharge from the wound being entirely free from fæces.

Considering the amount removed, and the impossibility of the sphincter being able to perform its normal function, this result—

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which is observed in the majority of these cases—is very remarkable, and may be taken as a strong argument in favour of the well-known view of O'Beirne—that the rectum is normally empty except immediately preceding the act of defæcation.

Upon examination of the structures removed it was found that a good margin of healthy tissues surrounded the disease. The piece measured about 3 inches in breadth and 2½ inches in length, and consisted of the anterior two-thirds of the rectal tube.

A section carried through both the principal growths shows that the greater portion of the smaller one is of a sooty black colour, while the larger one is quite white. The third and smallest one is also melanotic. Microscopic examination, kindly made by Dr. Abraham, shows that the growth is a typical sarcoma, much pigmented. In no part of it was there to be found any evidence of proliferation of the gland tissue of the mucosa, and, as far as could be made out, the disease originated in the submucosa.

Primary melanotic sarcoma is a disease of extreme rarity in the rectum, and, according to Virchow, this is the only portion of the intestinal tract in which it has been observed; and he points out the interesting fact, that although it is so rare in the human subject, it is of somewhat common occurrence in the horse.

At a meeting of the Société de Chirurgie, January 28, 1880, M. Nepvew delivered a lecture on the subject of rectal melanosis, and gave statistics of the cases which have previously been recorded, from which it appears that but ten instances had been noted. In only five of these was there any microscopic examination detailed, and all of these were instances of sarcoma. In two the position was immediately within the anus, once at the sigmoid flexure, and the rest were situate at the anus. In all the cases recorded the disease ran a rapidly malignant course, and in four which were submitted to operation recurrence was not long delayed.

The operative treatment of carcinoma recti may, with advantage, be classed under two heads—the one palliative, as directed only to the relief of the prominent symptoms of intestinal obstruction and pain, the other having for its object the complete removal of the disease. Of the former, three operations are at present practised

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where extirpation is inadmissible, and of these colotomy must still be ranked in the first place, although there seems to be a tendency amongst operative surgeons to make use, as far as possible, of other plans of treatment, even where the symptoms of severe obstruction are manifest. Of these operations the most important is linear proctotomy. This procedure, which has been so ably advocated by Verneuil as a substitute for colotomy in simple stricture, has also been strongly recommended by Kelsey as a method of treating obstruction due to malignant disease situated in the lower portions of the rectum; and he has still further advised the removal of some of the cancerous mass from the sides of the incision in order to give more room, or the making of a double incision and removal of the intervening structure with a like intent.

According to Verneuil, linear proctotomy, or external rectotomy, as it is sometimes called, from the analogy to external urethrotomy for urethral stricture, is best performed by entering the point of a benzoline cautery at the coccyx, and making it pass up outside the rectal wall to a point well above the disease. The bowel is perforated, and through the channel thus made a wire rope is introduced, the end of which is brought out through the anus. Both extremities are now attached to an écraseur, and all the tissues surrounded by the loop divided. This method has the advantage of being bloodless; but even when the section is made with the knife hæmorrhage is not troublesome if the incision is carefully made in the middle line, and, for many reasons, the bistoury is the preferable instrument. The entire section may be made with the Paquelin cautery, as recommended by Van Buren; or the galvanic écraseur, when at hand, will do the work in an admirable manner. Mr. Hamilton speaks very highly of it, and he recommends that the wire be first passed by means of a hollow needle.

The third operation is the removal, with a scoop or fingers, of as much as possible of the cancerous mass. Such cases are described by Allingham, Cripps, and Volkmann, and the result was a removal of the obstruction. From the recorded cases it appears that when

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the mass was thoroughly broken down and removed hæmorrhage was not excessive, and sometimes cicatrisation followed.

The defenders of colotomy hoped that it would prove of more utility than mere palliation, and that by diverting the flow of fæces, and so abrogating the physiological function of the rectum, atrophy and corresponding diminution in the malignant growth would result. Such, however, does not appear to be the case. It is also claimed for this operation that by preventing the flow of fæces over the ulcerated surface great diminution in pain would result; but we must remember that the rectum, from a very short distance above the anus, has but little sensibility—indeed, the early stages of cancer of the lower bowel are almost entirely devoid of pain, and it is only when the disease encroaches on the anus, or extends sufficiently to implicate the nerves of the sacral plexus, that pain becomes a prominent symptom. It is obvious, therefore, that, in the latter case, colotomy would be incapable of affording relief. To what cases, then, is colotomy still applicable? Where the disease is too extensive, or too high up, to be relieved by the other methods, where there is a communication with the bladder, colotomy is, undoubtedly, indicated; and both Bryant and Allingham have shown that the formation of a temporary artificial anus in cases of extensive non-malignant ulceration has, by affording physiological rest to the part, allowed healing to take place, after which the preternatural opening has been closed.

Where linear proctotomy is sufficient to overcome the obstruction it is much to be preferred to the formation of an artificial anus, as the risks of the former operation are small as compared with the many dangers inseparable from the latter, even in the hands of the most skilled colotomists, and the advantages of retaining the outlet of the intestine in its normal position are obvious. Moreover, although the sphincters are completely divided, much less incontinence results from linear proctotomy than from colotomy.

The radical cure of carcinoma of the lower portion of the colon may be attempted, as in the case described, by a free excision from the perinæum, or where the disease is situated high up the operation to which Marshall has given the name of colectomy may be per-

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formed. This procedure has been now practised at least twelve times, and of these, seven recovered from the operation, and the rest were rapidly fatal from peritonitis. There is, therefore, sufficient evidence to show that the operation should be entertained where the disease can be localised as a distinct tumour which is freely movable, or, at any rate, it is a strong argument for the adoption of the inguinal incision instead of the lumbar for colotomy, when, if the disease is not found suitable for removal, an opening in the gut can be made above it.

Excision of the rectum for malignant disease is now a well-established operation. Although portions of the rectum had been removed by operation before, Lisfranc was the first to bring it prominently into notice in 1833. It, however, fell into disrepute, and it is only within the last ten or twelve years that it has, to any extent, been revived, and at present is one of the subjects to which much attention is being paid in the surgical world, as is evinced by the important discussion which followed Esmarch's paper on the subject at the Copenhagen Congress this year.

In order to arrive at a just conclusion as to the advantages of extirpation of the rectum, it is necessary to review the course which carcinoma recti runs when not subjected to operation. It would appear, from a consideration of a large number of statistics, that the average duration of life is about two years from the appearance of the first symptoms, and during that time the condition of the patient is truly miserable. Where obstruction is present the constant straining is a source of perpetual pain and annoyance to the patient, and even when this symptom is not present the continued mucous and bloody discharge, the extreme pain suffered when the disease encroaches on the bladder, the anus, or the nerves of the sacral plexus, combine to render this disease one of the most distressing that can possibly come under the observation of the surgeon, and it is little to be wondered at that any operation which can hold out a chance of remedying this condition will readily be be adopted by both surgeon and patient. We must, however, consider the question from more than one point of view—first, as to the immediate risk to life; second, as to the probability of com-

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plete cure, and, if so, the condition in which the patient is left; and, lastly, supposing recurrence to take place, how long will it be delayed, and what will be the course of the secondary disease. I am convinced that a careful and unbiassed consideration of the facts bearing on these questions will serve to convince the impartial observer that they are not only sufficient to justify the operation in suitable cases, but that it is the duty of the surgeon to strongly recommend it.

Let us proceed to consider these questions in detail—first, as to the immediate risks of the operation. In trying to estimate the mortality of any operation, more particularly one which has of recent years only been extensively practised, as is the case with the operation under consideration, in this country at any rate, it is manifestly useless to collect all the cases published in the journals and from these deduce statistics, as there is a strong tendency amongst surgeons to publish isolated successful cases, while their fatalities are not so accurately recorded. Consequently, we must only place reliance upon the experience of those surgeons who give the results of the total number of operations which they have performed. I have collected 175 cases in which, I think, we may be satisfied that the conditions necessary for faithful statistics have been carried out, so that we may take the result as fairly reliable. These give a death-rate of 16·5 per cent., and, when we take into consideration the nature of the operation and the disease for which it is performed, we may consider this a fairly good result. Upon looking to the cause of death in these cases, we find that in upwards of 80 per cent. diffuse cellulitis, or peritonitis, is stated to have been the chief factor in producing the mortality. Although the full details of Listerian dressings are inapplicable to these operations, a great deal can be done in the way of antiseptic treatment to obviate these preventable complications; and the more fully we appreciate the advantages of thorough drainage and continuous irrigation, frequent washing with antiseptic solutions, or dusting with iodoform, the more likely are we to still further reduce this death-rate. When we consider the chances of permanent cure by the early extirpation of rectal cancer, we find very

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various opinions expressed by surgeons who, from the number of cases which they have had under their care, are capable of forming an opinion entitled to respect. Billroth, in 1881, had only two cases in which the patients lived two years after the operation; and Allingham speaks with great caution, apparently not considering that life is even prolonged by the operation; on the other hand, Cripps found, out of twenty-three cases, six died as a direct result of the operation, in nine the disease recurred after periods varying from four months to two years, and he was able to trace six that remained well at periods varying from two to four years; Curling had one case in which there was no return after six years; Velpeau records two cases which were well after ten years; and Verneuil and Chassaignac have had similar experiences; but, probably, the best statistics obtained by anyone are those of Volkmann. He states that three times he has had complete cures, and several cases of very late recurrence—once after six years, once after five years, and once after three. One died of carcinoma of the liver eight years after operation without local recurrence, and one case remained well eleven years after the removal of a very voluminous and high-reaching mass; in this case local recurrence, in the shape of hard nodules in the cicatrix, occurred twice, and were removed.

Dieffenbach records thirty cases in which the patients lived for many years after operation, but this statement is usually looked upon with suspicion. Sufficient has, however, been stated to show that the operation is a thoroughly sound one, as tending to prolong life, and in a few cases to be followed by complete cure.

As to the condition of the patient after recovery, we must remember the horrible disease for which it was performed, and compare the condition before and after removal of the disease. When the sphincter has not been removed the amount of incontinence is usually trivial, and it is only when there is diarrhœa that any trouble arises. This is generally easily met by the use of an antiseptic pad. When the entire lower end of the rectum has been removed a considerable amount of control often is maintained, but even in the worst cases of incontinence met with after amputation

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of the rectum the result compares favourably with the usual artificial anus following colotomy, and is vastly preferable to the state of a patient suffering from advanced rectal cancer.

Recurrence of the disease usually takes place as nodules in the cicatrix, or in the deep lumbar glands, liver, or other internal organs. When occurring in the cicatrix a secondary operation is often attended with good results. And even where not suitable for removal, these secondary growths are usually much less painful than the primary disease, owing to the destruction of the sensory nerves at the time of operation. Death from internal cancer is also considerably less painful than from unchecked carcinoma recti.

The best directions for the details of the operation are given by Volkmann (*Sammlung Klinische Vorträge*, May 13, 1878). He classifies the cases met with under three heads—1. Where there is a localised nodule of disease which can be removed by dilatation of the anus and the wound closed by suture: this is not attended with difficulty unless situated high up. 2. Where the greater proportion of the rectal circumference, including the anus, is diseased: in this case the anus must be surrounded by an incision extending into the ischio-rectal fossa, the rectum dissected up and amputated above the seat of disease. Volkmann, in the paper alluded to, recommends the bringing down and suturing of the divided rectal tube to the skin, drainage-tubes being inserted between the stitches. At the Copenhagen Congress, however, he did not appear still to advocate this procedure; and he now, therefore, agrees with the majority of surgeons of large experience in this operation, that the wound should be left to granulate. If sutured up, the strain is so great that the stitches rapidly cut out; but a very much more serious objection is that by so doing a pouch is found outside the rectal wall, in which, notwithstanding the most careful attempts to drain, fluids are apt to accumulate and become septic. It is astonishing how rapidly the process of healing by granulation takes place in these cases, and as it progresses the mucous membrane is drawn down, so that, eventually, but a small cicatrix results.

Volkmann's third class is where the disease is altogether above

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the anus, involving the entire circumference of the bowel. A deep posterior incision to the coccyx is the first essential procedure in this instance; the rectum is then incised round its circumference above the external sphincter, the tube dissected up and amputated as before. In this case, again, it is better not to make any attempt to suture the divided ends. And the posterior incision should be carefully kept open to provide sufficient drainage. This operation is open to an objection not present in the other two—namely, that as the blood-vessels supplying the lower portion of the rectum are of necessity divided, gangrene of this portion is apt to occur. Where the disease is extensive, wound of the peritoneum is sometimes a serious complication of the operation—it has happened when less than three inches of rectum have been removed; while, on the other hand, upwards of five inches have been removed without this membrane being seen. All hard-and-fast rules as to the distance of the peritoneum from the anus, based on anatomical measurements, are manifestly fallacious, for, in the first place, the anus is so movable that the distance may vary considerably in the same individual, according as the anus is protruded or retracted; and in cases of cancer the constant straining and involvement of the peritoneum in the diseased mass will tend materially to alter the normal relations. Should the peritoneal cavity be opened, the wound ought at once to be plugged with a septic sponge, and after the removal of the disease the sponge is to be removed and the opening carefully closed by fine catgut. Involvement of the peritoneum has an importance quite apart from the immediate and grave danger of septic peritonitis, as it usually indicates such an involvement of the lymph paths in the malignant disease that recurrence of the growth will not usually long be delayed.

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THREE CASES OF OVARIOTOMY.

By WILLIAM THOMSON, M.A., F.R.C.S.;
Surgeon to the Richmond Surgical Hospital, Dublin;
General Secretary to the Academy of Medicine in Ireland, &c., &c.

[Read in the Surgical Section, December 12, 1884.]

The subject of ovariotomy is one of such varied interest that for a long time to come it must continue to attract our attention. It is but the other day since its opponents were numerous and strong, and their obstinate refusal to countenance it or practise it seemed justified by the terrible mortality which attended its performance; yet now we count the cases by thousands, and with the increase of operators the death-roll has gone down steadily until it has reached a percentage which excites our admiration and amazement. What the causes are of this great revolution in abdominal surgery I will not now stop to consider in detail, but they may be summed up as in the first place due to improved methods of operating, and in the second to the attention which is now given to cleanliness in wound-treatment. Whether the surgeon adopts the Listerian principles or dressings, or does not, of this there can be no doubt, that in this department he tries to secure, perhaps more scrupulously than elsewhere, a cleanliness which even a few years ago would have been laughed at. The scoffers at Lister and his method have, at all events, been unable to hold out against that essential part of his doctrines, and at least the coarser parts of surgical dirtiness have been recognised as grave dangers to the patient, and have been got rid of.

The cases which I have to submit to this Section of the Academy are very few in number, but they are all I can report at present; and as they were done under circumstances which have by some

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been held to be risky, I think we may gain something from a very brief record of them:—

Case I.—M. F., aged forty, married; sent to me by Dr. R. Saunderson, Rhode, King's County; admitted to the Richmond Hospital, June, 1878. She had had four children, and the tumour had been growing about two years. Fluctuation distinct. The tumour had begun in the left side; girth round the umbilicus, 40 inches; catamenia regular. The operation was done on the 23rd of June under spray; there were no adhesions; the pedicle was secured with silk, and the stump dropped into the pelvis. The fluid contents of the cyst amounted to about 12 pints. The wound was dressed next day, and then not for four days, when all was found to be united. The stitches were removed on the eighth day. The highest temperature was 101°. No symptom gave the least alarm, and the patient left hospital in five weeks, and returned to the country.

Case II.—B. M., aged twenty-five, unmarried; sent to me by Dr. R. Saunderson, Rhode, King's County; admitted 25th June, 1884. Patient thin, sallow, healthy; catamenia regular; some irritability of bladder; digestive, nervous, respiratory, and circulatory systems healthy; first signs of disease three years ago; no pain at any time; girth round umbilicus, 34½ inches; from ensiform cartilage to umbilicus, 7½ inches; from umbilicus to symphysis, 9 inches; from right anterior superior spine of ilium to umbilicus, 9 inches; from left, 9¾ inches. Operation performed on July 16. Eleven pints of fluid drawn off; no adhesions; pedicle on right side ligatured. Five sutures were inserted. The wound was dressed in forty-eight hours, and again on eighth day, when catgut sutures were removed. Wound quite healed. Highest temperature, 100·8°. Was allowed up on fifteenth day, and was discharged on the twenty-ninth day, well.

Case III.—M. L., aged thirty-two, unmarried; sent to me by Dr. Rutherford, of Ballinasloe; admitted November 5, 1884. Patient tall and well made; says she noticed the tumour beginning at her right side about two years ago. The swelling is about the size of an adult head. Fluctuation very faint; has had pain occasionally since March, 1883. The patient was seen in consultation by Dr. Kidd, who agreed as to the diagnosis. The operation was performed on the 14th November, 1884. There were adhesions to

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the anterior abdominal parietes, but they were easily broken down with the finger. The tumour was then tapped, and 2½ pints of fluid were withdrawn. This was black and coffee-like. The tumour grew from the right side. The pedicle was long, and was treated in the usual way. The highest temperature was 100°, and that only once. The pulse never rose above 80. The patient left bed on the fifteenth day, and is now—twenty-ninth day—leaving the hospital.

These cases, as I have said, are all in which I have performed the operation of ovariotomy, and they have been uniformly successful. The list is certainly not a very formidable one, looking to the large returns which are frequently published by some surgeons who specially devote their attention to this branch of surgery in England and on the Continent; but there are some reasons, to which I shall refer later on, why a general surgeon like myself should record his results, whatever they may be, in this department. Ten years ago any surgeon here would have justly exulted in the results I have noted, for the fatality then was great and almost deterrent; but we have learned, as all must learn, from the experience of others, and the improved treatment of wounds has helped, perhaps more than anything else, to establish the procedure as one of the least fatal of major operations.

All my cases, as were most of those of my colleagues, were operated upon in one of the medical wards separated from the surgical hospital. Each patient was given into the charge of one of the surgical nurses, who never left her until the termination of the case. My pupil gave up attendance upon other cases, no matter of what description. The patient was prepared by having a warm bath the night before, the bowels were cleared out, and the operation was performed at ten in the morning. The abdomen was cleansed with soap and water, and immediately before the incision the skin was washed with ether. The sponges were always new, and were prepared under my own supervision by steeping in a weak hydrochloric acid solution for twenty-four hours. They were then washed in dilute Condy's fluid, and next in a mixture of sulphurous and of oxalic acids. This bleached them thoroughly.

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They were next put into a two and a half per cent. solution of carbolic acid for twenty-four hours, and were ready for use. The carbolic spray was allowed to play for about half an hour before as well as during the operation, and when the patient was completely anæsthetised the visitors and students were admitted.

This practice is one which is condemned by many ovariotomists, who confine the spectators to half a dozen in number at most; but with antiseptic precautions, and judging from my own and the cases of my colleagues, who have not lost one case from septicæmia, there does not appear to me to be any good reason for departing from it. If, as we shall presently see, so much stress is laid upon "experience" in this operation, there surely is the most powerful argument for the admission of students. So far, at all events, I have seen nothing to induce me to foster any idea of mystery about ovariotomy by doing the operation with "closed doors."

The pedicle was in each instance tied with carbolised silk. A needle was passed through the thin tissue, the two portions were tied separately, and then the whole circumference of the pedicle was included in a third loop. The tumour having been separated, the stump was dropped into the abdomen, and the wound closed after a final examination and mopping out of the pelvic cavity. No drainage-tubes were used. There were three dressings—the first in forty-eight hours, the second usually on the eighth day, when the carbolised sutures were removed, and the last about the tenth, when, the wound being firmly united, the gauze dressing was given up altogether. The patients were allowed to leave bed on the fifteenth day, and usually went home in about four weeks from the date of operation.

The conflict of opinion as to the treatment of the pedicle which for some years raged has ceased, and it has now come to be the accepted practice in the majority of cases to apply the ligature and return the stump into the abdominal cavity, instead of using the clamp and securing the stump surface external to the superficial wound. The experience of Keith, Knowsley Thornton, and Lawson Tait, has been altogether in favour of the intra-peritoneal method.

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Tait has argued very forcibly that the mortality of the operation was largely caused by the clamp, and his statistics and those of other surgeons go to justify that opinion, but at the same time it has not been by any means the sole cause. In the cases collected by Baum it appears that the mortality where the ligature was used was greater than with the clamp. Thus, in 875 cases in which the pedicle was clamped, 21·25 per cent. died; while in 746 in which the pedicle was ligated and dropped into the pelvis, 32·17 per cent. died. Such a high mortality as this, however, must have depended upon other causes than the mode of treatment of the pedicle—on hæmorrhages, septicæmia, shock, and the many accidents which may attend any wound. On the other hand, Keith, Thornton, and Tait, who all tie the pedicle, have reduced their mortality to 5 and 3 per cent.—a series of results which are certainly unsurpassed in any department of major surgery. The return of the pedicle to the pelvis seems to be the natural and most scientific way of dealing with that part. To secure the surface of the stump in the abdominal wound, to leave it free to the accidents of an exposed cut surface, and thus to permit of the introduction of septic material into the peritoneal cavity, would appear to be one of the most certain ways of arriving at fatal results.

The drainage of the pelvic cavity after abdominal section is a further question of great moment. The fact that Keith practises it, and that Knowsley Thornton and Tait do not, shows a wide and remarkable difference of opinion among eminent specialists. It may be that in certain cases it is of advantage to have a drainage-tube in the pelvis, but in cases which are uncomplicated by many adhesions and consequent risk of secondary hæmorrhage it is unnecessary, and it is at all times a trouble and an absolute danger. The dressings require frequent changing, while everything should be left as quiet as possible; there is always risk of infection of the fluids when the tube is exposed to the air, and the tube itself must be a cause of irritation to the very delicate and sensitive structures with which it lies in contact. In 1881 Knowsley Thornton published a record of 150 cases. In these he drained twelve times, and lost 25 per cent. Four years ago Dr.

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Bantock, of London, published 75 ovariotomies, in which he drained eighteen times, and lost 27·77 per cent. In none of my cases was it used at all, because there were no adhesions sufficient to give rise to any serious hæmorrhage, and in none was there any symptom to make me regret that I had not used it. Our practice ought to be directed, as Mr. Thornton very properly says, to its abolition altogether; and although we may be prepared to admit with Mr. Tait that now and again severe cases will arise in which its use is necessary, we should direct every effort to avoiding its employment by securing all bleeding points, and thoroughly drying the cavity.

In Case No. III. the tumour was small, and when the patient was dressed she did not present any evidence of its existence. The question arose in consultation whether it ought to be interfered with, especially as its growth was not very rapid. There are cases, of course, in some of which increase of size is slow; in others the tumour does not appear to enlarge at all beyond a certain point, and gives little or no inconvenience to the patient. But when we consider the probabilities of the progress of these growths, their liability to contract adhesions to the abdominal walls and viscera, and thus to increase immensely the risk to life should operation ultimately be undertaken—when we further think of the painful suspicions that so often attach to the subjects of these tumours, it does appear to me that, except under very special circumstances, relief ought not to be delayed. In uncomplicated cases the danger to life from operation is now very slight, and the sooner we anticipate difficulties in its performance the better.

I ought perhaps to close with these remarks upon the cases themselves, but the question has been very forcibly put to me in Mr. Lawson Tait's book—Ought I to have operated at all? Was I guilty of gross presumption, and did I unduly risk my patients' lives through my ignorance and want of experience? That is the charge which is brought against any general surgeon who ventures to intrude into the sacred precincts of what is now called specialism in abdominal surgery. Lest I should be regarded as overstating,

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let me quote Mr. Tait's own writing under the index-question—Ovariotomy: Who is to do it?—"I am also strongly of opinion that no surgeon engaged in constant attendance on the promiscuous cases admitted to a general hospital should perform such an operation as ovariotomy, and I look on it as mere foolhardiness on the part of anyone to perform it to whom it will probably never occur again to engage with such a case, or whose experience is likely to be limited to two or three such cases in a lifetime. It is an operation beyond all others requiring that readiness of adaptation for emergencies which wide experience alone can give. Its complications are far more varied and tax far more heavily the courage and presence of mind of the operator than those of any other operation in surgery, and one or two successful cases scarcely compensate for those which are unsuccessful by lack of experience."[a]

I admit that this is very astute, but it is not very flattering to my common sense to ask me to believe it. The assumptions are wrong and the reasoning is fallacious. May I ask upon what special grounds a general surgeon is to be excluded from operating upon the peritoneal cavity? Is it because of the danger of conveying infection? Then with much more reason he ought never to operate upon any other part of the body. The surface of a stump after amputation is much more susceptible of mischief than is the exposed peritoneum. If it be said that there are wounds in the divided pedicle, and the parts from which the adherent surfaces have been separated, weeping blood or serum into the abdominal cavity, it does not destroy my position. Upon this point Sir Joseph Lister puts the case clearly and truly:—"One great peculiarity of operation wounds within the abdomen as compared with those in ordinary situations is that the plasma from the cut surface is poured out into a large cavity lined with a serous membrane disposed to absorb it as fast as it is effused. Thus, without drainage or any outlet whatever for discharge being provided, the serum is, under favourable circumstances, prevented from accumulating, as it would in ordinary wounds similarly treated, and

[a] Diseases of the Ovaries. P. 256.

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opportunity is not afforded for putrefaction."[a] Mr. Tait's doctrine, then, I submit, lands him in an inconsistency.

But, then, we are told that this "is an operation beyond all others requiring that readiness of adaptation for emergencies which wide experience alone can give." Now, so far as an uncomplicated case of ovarian tumour cyst is concerned, there are few things in surgery easier than its removal. The complicated cases are in the minority, and it is upon this smaller number of cases presenting difficulties, little and great, that Mr. Tait bases his warning. Does he seriously think that the possible complications of one of these cases may unnerve anyone but a specialist? Surely a surgeon in a general hospital, who is daily brought face to face with emergencies, whose wits and judgment are trained, by frequent exercise, to quick decision and action, may be intrusted with the treatment of adhesions of any kind in an abdominal case. To talk of excluding the general surgeon is to show forgetfulness of this fact, that it is from his enterprise and daring that the greatest advances in abdominal surgery have taken their start. When I mention incision of the kidney and removal of it, excision of portions of the stomach and the intestines, and suture of the bladder, as among his achievements, I think I may fairly claim for the general surgeon that *he has* "a readiness of adaptation to emergencies" with which the possible complications of an ovarian case cannot for a moment be compared.

But another ground of exclusion is want of experience. Says Mr. Tait:—"One or two successful cases scarcely compensate for those which are unsuccessful by lack of experience." Now, an argument built upon such a statement as this unfortunately proves too much. Everyone admits the advantage of experience in all cases; but if it be said that one is never to operate until he has personal experience, the absurdity is obvious. Everyone must have his first case, and in order to do the best for it he avails himself of the recorded experience of others. Even Mr. Tait must have followed this course. No one can tell whether he will have one, or twenty, or a hundred cases of ovariotomy; and a surgeon

[a] Address at International Congress, London, 1881.

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would be doing himself wrong, and even on Mr. Tait's showing, endangering some future patient's life, if, when the first case of abdominal section presented itself, he declined to operate because he thought he would never meet with any more.

Finally, Mr. Tait quotes Sir James Simpson to this effect:—"It was perhaps the only capital operation in which the surgeon was required to proceed upon the diagnostic knowledge of another party." This observation may have been true at the time it was written; but certainly there are few surgeons who are not able to diagnosticate an ovarian or a uterine tumour. It is not always easy, I admit, and even the specialists have to make exploratory incisions when in doubt. If Mr. Tait has to open the abdomen in a particular case to determine the position and character of a tumour, he is in no better position than myself, when I can explore the cavity with my finger. We are always glad to have the advantage of the opinion and advice of our distinguished gynæcological colleague, but I am sure he will not contend that the diagnoses were made solely by him. Even if Mr. Tait spoke correctly in this, it would not follow that the case ought to be given to the man who can diagnosticate it clearly. Diagnosis is but the first step, and there is much more than that necessary before we reach the position of the successful operator.

I have perhaps occupied more time than I ought in thus dealing with Mr. Tait's reference to general surgeons. But it was made once publicly in this hall, by himself, and it has been given all the circulation of his very valuable book on "Diseases of the Ovaries." Moreover, Mr. Tait is not the only one who holds the same opinion, and it is time, I think, that some protest should be made against a doctrine which is founded upon inaccuracies, and which has a tendency to growth. For myself, I do not care what line of practice a man may take, if he finds that the course of events leads him to it exclusively. But there are daily-increasing attempts to parcel out the body into special districts, and to put up notices declaring that no one who does not devote his whole attention to some one of them may venture into it. It is to this only I object. There is no reason why a general surgeon should not enter upon

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any or all of them, if he feels that his knowledge justifies him—nay, more, I would say that his familiarity with wounds and their treatment, and with the risks and dangers that lurk about them, his training of mind and eye and hands, all point to him as specially competent to deal with abdominal surgery.

NOTE.—Since the reading of this paper abdominal section has been performed in three cases. Two patients have been discharged well, and the third is under treatment.

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ON THE TRANSPLANTATION OF SKIN FLAPS WITHOUT PEDICLE FOR THE CURE OF CICATRICIAL ECTROPION.

By H. R. SWANZY, A.M., M.B., F.R.C.S.;

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[Read in the Surgical Section, January 23, 1885.]

The transplantation of skin flaps, without pedicle, from distant parts of the body for the relief of cicatricial ectropion, known as Wolfe's method, is one with which the members of the Section at large are already acquainted, so that it is needless for me to point out the advantages it possesses over other methods of dealing with these cases. However, in connexion with the exhibition of four eyelids (in three patients) operated on in this way, I may be permitted briefly to draw the attention of the Section to the procedure—the more so as there are some points in it which I think deserve greater attention than they have generally received.

My own experience of the method is of recent date, and the total number of my operations is seven. These seven operations have been performed in four patients, of whom you see three here to-night. The early accounts of the results of these operations were not to me sufficiently attractive to induce me to abandon the old plans in its favour, and it was not until forced by the circumstances of a case that I employed it. Now, having found it a most satisfactory proceeding, I much regret that I postponed its trial so long. I believe, also, that it will yet be a more perfect method than at present.

Before briefly narrating my own cases it may be convenient to recapitulate the steps of the operation. They probably vary somewhat with the operator, but are, in general, as follows:—Let us

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suppose the upper eyelid to be everted in consequence of cicatricial contraction of its skin, so that the free margin of the lid has become incorporated—as is often the case—with the eyebrow, and the conjunctival surface of the lid has become exposed. To remedy this condition an incision is made through the skin above the position of the adherent ciliary margin, and extending, not only the whole length of the latter, but some 5 mm. further at either end. The eyelid is now dissected down, and reduced to its normal position. By this means a round or oval bleeding surface of about 5 cm. diameter is produced. The margin of the lid is secured to the margin of the lower lid, or to the cheek, by two or three points of suture. The dimensions of the wounded surface are then measured, and its size, with the addition of one-third in every diameter, is marked out with pen and ink on the skin of the inner surface of the forearm, or other selected place. The portion of skin thus mapped out is dissected off with as little of the subcutaneous connective tissue and fat as possible, care being taken not to contuse the flap. Immediately on its removal the flap will be found to have shrunken to two-thirds of the size which was marked out on the arm, and hence the reason for having made that area one-third larger than the wounded surface which is to be covered. The flap is now laid, with its epidermic surface below, on a warm plate, and every morsel of subcutaneous connective tissue and fat is carefully removed with a curved scissors, so that only true skin and epidermis is left in the flap. While the flap is being prepared, the wounded surface on the eyelid has remained exposed to the air, in order that all oozing of blood from it may be arrested, and by the time the flap is ready for application the wounded surface of the eyelid will be found with a well-known glazed appearance. If there should still be a slight bleeding, the surgeon must wait until it ceases. The flap is then applied to the wounded surface, and is retained in its position by interrupted sutures around its margins, care being taken, above all things, that there be no stretching of the flap, while, also, its margin should not extend beyond the margin of the wounded surface. When fixed in its place the flap presents a ghastly white appearance. The dressing is applied and

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kept in position by a roller bandage round the head calculated to give merely a gentle pressure on the eyelid. Such an operation, when the surgeon himself prepares both the eyelid and the flap, as I prefer to do, occupies from 1¼ to 1¾ hours.

The dressings are not removed for four or five days. If all have gone well the white appearance of the flap will then be found altered to a pale pink, while union will seem to have taken place around the margins. If the various parts of the flap be touched they will be felt to be firm and adherent to the surface underneath. At some of the points of suture a slight suppuration, or it may be a slight sloughing, is very apt to be present.

But a portion of the flap may be found to be gangrenous. Sloughing of even a third or more of the flap may take place, and still a very fair therapeutic result be ultimately attained.

Five or six days after the operation, in most cases, the epidermis of the flap becomes loose and soon peels off, leaving the highly vascularised rete Malpighii exposed, and presenting the *appearance* of a healthy granulating surface. This very soon begins to have a cuticle formed on it, and it finally becomes healed all over in the course of a fortnight or three weeks.

If things would now remain *in statu quo* both patient and surgeon would, as a rule, be content, but, unfortunately, a change soon commences which must always be calculated on—namely, a shrinking of the flap. This shrinking goes on very slowly, and does not attain its utmost limit for many months, by which time the flap will sometimes be found to be reduced to a hard lump of apparently cicatricial tissue, such as is seen in the lower lid of the old man (exhibited). This, however, in most cases, is sufficient for the purpose for which the operation was undertaken.

With regard to my own cases:—The *First Operation* was performed early in the year 1883 on the upper eyelid of a man, which was ectropionised from a burn. No plastic operation with use of flaps from the face would have been possible. I was assisted by Dr. Franks, who dissected the skin flap from the forearm, while I prepared the eyelid. The flap was secured in its place with a continuous fine wire suture, and dressed with goldbeaters' skin and

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carbolised oil on lint. About one-third of the inner end of the flap sloughed. The epidermis peeled off. There was a good therapeutical result so long as the case remained under observation, which, however, was not for more than two or three months.

The *Second Operation* was on the old man (aged sixty-five), whom you saw outside. His under lid of the left eye was turned out in consequence of a burn, and had been operated on by the President of this Section (Professor Bennett) at Sir Patrick Dun's Hospital a short while before by Mr. Wharton Jones' V.Y. operation, as well as I could make out from the cicatrices. Knowing the operator, I can safely presume that that operation, as such, was complete in every detail, and yet the ectropion returned. The man then came under my care at the National Eye and Ear Infirmary, and on the 27th of last February (1884) I transplanted a portion of skin from his left forearm to the eyelid. The eyelid was dissected up as far as it was possible, so that the free margin reached far above the position it should normally occupy, and was sutured above the eyebrow on the forehead. In this way a very extensive wounded surface was produced. It measured about 5·25 cm. by 2·35 cm. The area marked out on the arm measured 7 cm. by 6 cm. There were a dozen silk sutures used in securing the flap in its new position. The dressing consisted of goldbeaters' skin and carbolised oil on lint. The result was that a small portion of the flap at the inner end sloughed; there was some suppuration at several of the points of suture; and the epidermis peeled off. At present the eyeball is fairly protected, and the patient himself is much pleased with the result; but at the inner end of the lid, where the sloughing took place, there is a hiatus, which leaves the caruncle exposed, and is the cause of much tearing. I propose admitting him to hospital again to endeavour to relieve this. The flap attained its utmost degree of shrinking four or five months after the operation, and since then remains as you have seen it. It now measures 2½ cm. by 6 mm., and hence the shrinking has been to the extent of about 55 per cent. of its original size when transplanted, but this includes the part which sloughed.

The *Third Operation* was on the girl, aged nineteen, whom you

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have seen without. Her nose, all her eyelids, the whole of her forehead, and the front part of her scalp had been burnt by her falling into the fire in an epileptic fit in April, 1883, about ten months before her admission to hospital. When I first saw her, her forehead was a continuous granulating surface, and each of the four eyelids, but more particularly the upper lids, were everted, so that the eyes could not be closed. Having, by a lengthened treatment of skin-grafting after Reverdin's method, nearly brought the forehead to healing, I transplanted skin from her left forearm to her left upper eyelid for cure of its ectropion. The plan I tried here was rather that of Stellwag and De Wecker than of Wolfe. In it the flap is not applied in one piece to the eyelid, but is cut up into four or five morsels, which are laid on the wounded surface so as to cover it completely, their edges touching. The advantages claimed for this method are—that these small flaps adapt themselves more readily to any unevenness of the wounded surface; that they require no sutures; and that if one or two of them should slough the existence of the remaining ones is not jeopardised, as is the case where a small portion of the larger flap mortifies. The dressing employed was the gold-beater's skin next the flaps and carbolised oil on lint over that. On removing the dressing on the fourth day all the flaps of the skin were found loose and sloughing, and bathed in pus. The wounded surface of the lid granulated, and in spite of much skin grafting, it finally contracted to such an extent as almost to nullify any result from the operation.

The *Fourth Operation* was on the woman, aged forty-five, whom I have shown you. She was operated on on the 18th of April last. Her right under lid was everted from a burn to such a degree that its ciliary margin was found adherent to the cheek low down. The lid was dissected up very completely, so that when reflected its free margin came in relation with the skin above the eyebrow, and covered up the eye and upper eyelid, and I fastened it to the skin of the forehead by means of three points of suture. The wounded surface was consequently a very extensive one. It measured 3·25 cm. by 3·5 cm., and the portion of skin mapped out on the right forearm was rather more than one-third larger. The flap

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was fastened in its new position in one piece by a dozen interrupted silk sutures around its margin. The dressing consisted of simple vaseline at first, and afterwards of vaseline with boracic acid, spread on lint, and over that cotton-wool with a roller bandage. On removal of the dressings on the fourth day the flap was found adherent all over and around its margins, while there was a little suppuration at some only of the points of suture. The sutures were removed, and the case progressed without drawback of any kind. The epidermis did not peel off, and consequently there was no exposure of the rete Malpighii, and no highly vascularised granulating condition of it; and owing, I believe, to these circumstances, the subsequent shrinking of the flap was much less than in the case of the old man. The complete shrinking was attained in from three to four months. The flap measured then, as it measures now, 2·75 cm. by 2·5 cm., the shrinking, therefore, having been only about 35 per cent. of the size of the flap when first applied. The appearance of the flap now is very superior to that in the old man, for it is not very different in colour from the skin of the cheek, and to the touch it is soft and pliable, and not hard and gristly. Therapeutically the result is good, also, except that the lid stands out from the eye a little—a defect which a simple operation could remedy.

The *Fifth Operation* was performed on the right upper eyelid of the girl exhibited, on whose left upper eyelid I had already unsuccessfully operated. This operation was done on the 30th April last, the flap being transplanted in one place from the right forearm and secured with about a dozen catgut ligatures. I took pains here, also, to produce as large a wounded surface as possible, so that the free margin of the lid reached down over the under lid, and was fastened to the cheek without the slightest tension. The dressing used at first was simple vaseline on lint. On the fourth day, on removing the dressings, all the sutures were found to be dissolved or to have loosened their hold, and the flaps had become slightly shifted over the underlying surface. Its colour was bad; there was some discharge; and I had little hope of a good result. After one or two more dressings it was evident that the whole flap

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was necrosed. The wounded surface of the lid granulated, was treated with skin grafts, and finally shrank to much its former condition, with similar degree of ectropion. I referred the failure in this case to the sutures being of catgut, which did not hold long enough to prevent shifting until union would take place. This patient was sent home to the country soon afterwards for three months. She returned at the end of October, and on the 17th of November, three months ago, I operated again on her right upper eyelid, making my—

Sixth Operation.—The eyelid, of which the free margin, with all its eyelashes preserved, was incorporated with the eyebrow, was dissected down to the utmost degree possible, so that it quite overlapped the underlid. The free margin was as usual fastened to the cheek. The wounded surface measured 5 cm. by 3 cm. The flap taken from the inside of the right forearm was one-third larger before being dissected off. I employed a great number of fine silk sutures—26 in all—at a distance of about 3 mm. from each other all the way round the flap to keep it in its place. The dressing consisted of carbolised oil on lint immediately over the flap, and cotton-wool and a roller bandage. On removing the dressings on the fourth day the flap was found to be looking well, except at one spot about as big as a threepenny-bit near the inner end, which was of a purple hue and slightly raised. The dressing with carbolised oil was renewed every day. The slough became more pronounced, but did not extend further over the flaps. A little suppuration took place at nearly all the points of suture, most of which latter I removed on sixth day. Many of these little points of suppuration, especially along the upper margin of the flap, became confluent, so that a line of suppuration was there formed. On the seventh or eighth day the epidermis peeled off, and the underlying rete Malpighii became exposed in its highly vascularised condition. In the course of a few days more this surface began to be covered with a new cuticle, and the slough, having come away from the necrosed spot, it, too, cicatrised over. You have seen the present condition of the lid. Although there may still be some further shrinking, I look forward to a good therapeutical result here.

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Seventh Operation.—On the 19th of last month (about five weeks ago) the left upper eyelid of the same patient was operated on for the second time. When the lid had been dissected down and its margin secured to the cheek the raw surface was found to measure 5 cm. by 3 cm., and I traced out a portion of integument on the arm measuring 7·5 cm. by 4·5 cm. for transplantation. In dissecting down the lid a small arterial branch was divided at the upper and outer orbital margin, but the bleeding was easily arrested by torsion. In the course of this dissection an accident occured twice, which I had not before experienced, although I had always feared it might occur—namely, a button-holing of the eyelid. I made one button-hole about 8 mm. in diameter near the upper and outer margin of the wounded surface, through which some orbital fat protruded, and another smaller hole in the lower and outer quarter of the wounded surface, opening to the conjunctival surface. I do not think these two accidents were due to want of care on my part, but I regard them as almost unavoidable, in consequence of the thinness of the applied surfaces, which I was endeavouring in this case to separate. These button-holes do not seem in the sequel to have interfered seriously with the result of the proceeding. Sixteen fine platinum wire sutures were employed to secure the flap in its new position, and the eyelid was dressed with sero-sublimate gauze moistened in a solution of corrosive sublimate of the strength of 1 in 2,000.

Dec. 24th (fifth day).—First dressing. The whole epidermis was found peeling off, and there were two or three small purplish spots near the lower margin of the lid, in the centre of it. There was no suppuration at the sutures. There was some slight purulent discharge coming from under the epidermis. The silk sutures fastening the lid to the cheek and six of the wire sutures have been removed. Dressed with sero-sublimate ointment.

26th (seventh day).—The purplish spots had not extended. At the inner corner of the flap, where, owing to a depression in the wounded surface, corresponding to the sulcus at the inner canthus, it was not closely applied, there is a considerable amount of puru-

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lent matter. The remaining wire sutures were removed, and the eyelid was dressed dry with sero-sublimate powder.

A few days later, some luxuriant granulations having sprung up at the inner angle, and one near the centre of the flap, probably where one of the button-holes was situated, these were touched with solid nitrate of silver. A new cuticle formed on the transplanted flap in the course of a few days, and the granulating points healed over. The complete result, as you have seen, is fairly satisfactory, although there has already been a good deal of shrinking, and I fear there will be more. Should this go too far, it will be necessary to do a supplementary transplantation.

In addition to the points generally regarded as important for the success of the operation, I would point out the following:—1st. It is desirable that the wounded surface on the eyelid should be made as extensive as possible, the dissection for the purpose of reducing the lid into position being carried to the farthest possible limit. The plan usually recommended is to carry this dissection only so far as to bring the free margin of the eyelid being operated on into contact with the free margin of its fellow; but, as the members will have observed, I have gone so far with the dissection in many cases, that the free margin of the lid was fastened on the forehead or on the cheek, as it happened to be the under or upper lid, without any tension whatever. The reason for forming an eyelid so very much too large in the first instance is that the inevitable shrinking may not ultimately reduce the eyelid to less, or much less, than its proper dimensions. If, in the first instance, the eyelid be made but little larger than normal, the shrinking of the flap must necessarily reduce it later on to much less than its normal size. I am not aware that this point has been before brought forward.

2ndly, with regard to the sutures. There can be no doubt it would be much better if these could be done without, as they nearly always cause some suppuration. In his first operation Wolfe used no sutures, but dissected up the margin of the skin round the wounded surface and slipped the edge of the flap under this, as a watch-glass is put into its frame. He seems of late to have returned to this practice. I have not as yet any experience of it. I would not

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again use catgut ligatures, as they seem not to hold long enough to perform the office required of them; nor would I again use a very large number of sutures, even of fine silk, because they give rise to a continuous line of suppuration around the margin, and, moreover, so many are really quite unnecessary. I thought the fine platinum wire sutures caused less suppuration than the silk. If sutures are used I fancy the best plan will be to apply only so many fine silk sutures as are just sufficient to keep the flap fixed accurately in its place.

3rdly, with regard to the dressing. I believe it to be a mistake to use carbolic acid in any form in connexion with the proceeding, and I say this although I have used it in all my cases but the last. I do not in the least share Dr. Wolfe's[a] remarkable opinion that the "so-called antisepticism," as he terms it, has no place in ophthalmic surgery, but I do think that carbolic acid is unsuited to the method of which we are now treating as being apt to irritate the delicate skin flap, and I feel pretty sure it increases the liability to peeling off of the epidermis. The dressings and lotions should be antiseptic, but quite unirritating. Boracic acid and sero-sublimate dressings are very suitable.

Finally, I am inclined to think that where the operation is so successful that the epidermis has not peeled off, the shrinking is slight in comparison with the cases where it does come away, and the subjacent rete Malpighii has to throw out a new cuticle.

[a] Centralbl. f. d. Prakt. Augenheilkunde, January, 1880.

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ASTRAGALOID OSTEOTOMY IN THE TREATMENT OF FLAT-FOOT.

By WILLIAM STOKES,
Professor of Surgery, and Vice-President, Royal College of Surgeons.

[Read in the Surgical Section, January 23, 1885.]

For some time it has been a matter of doubt to me whether the generally held theories as to the ætiology of pes planus or flat-foot were such as should be unhesitatingly accepted. These have been twofold—one based on an alleged relaxation of certain ligamentous structures, notably the calcaneo-scaphoid ligament; and the other that it is largely, in some cases entirely, due to a paralytic condition of certain muscles connected with the ankle and foot, more especially the tibialis anticus.

My doubts as regards the value to be placed on the first of these theories have arisen mainly from there having been up to this no satisfactory demonstration of the existence of ligamentous relaxation; from observing that in a large proportion of the cases that have fallen under my observation the deformity occurred, not in delicate, weakly, anæmic subjects, but in young persons who were strong, healthy, and well nourished; and from noticing how uniformly unsuccessful the attempts are to permanently remedy the deformity by any of the routine lines of practice, such as pushing up the arch of the foot and keeping it in that position by any of the various mechanical adjustments designed for this purpose.

In these cases, too, there has been an entire absence of any evidence of local paralysis.

These facts have naturally shaken my belief in the views as to the ætiology of the condition which are usually taught, accepted, and acted on by surgeons.

However, not having until recently an opportunity of either

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verifying or disproving these doctrines by means either of dissection, or inspection in pathological museums of examples of the deformity—for, strange to say, although it is a defect of such common occurrence, the pathological museums both here and in London are singularly deficient in examples of it—I consequently was unable to arrive at any definite conclusion as to what should or should not be accepted as regards the causation and pathology of the deformity.

My attention was comparatively recently directed again to the the subject on reading Professor Ogston's paper on flat-foot—one written with that ability and originality which characterise his contributions and make them so grateful to the surgical student.

In this communication he advocates a complete revolution in the previously accepted methods of dealing with the deformity—viz., cutting down on the astragalo-scaphoid articulation, removing a thin slice from the surfaces of these bones where they articulate with each other, restoring the arch of the foot, and fixing the two bones together with fine ivory pegs, which are left in the wound. This procedure has been attended, it is stated, with eminently satisfactory results. There are, however, two features in it which appear to me to detract somewhat from its merit—one, that the operation is so complicated and difficult; and another, and more important, is that one of the articulations in the tarsus is necessarily completely obliterated, an object of the operation being to get, after reducing the deformity, anchylosis of the astragalo-scaphoid joint.

Before discussing the method of operating adopted in the case I desire to record, I may previously advert to the views already alluded to in reference to the ætiology of the defect. As regards the opinion emphasised by Professor Sayre, and, I am informed, endorsed by Mr. Barwell, in reference to a paralytic condition of certain muscles, notably the tibialis anticus, I would say that, without denying the possibility in certain cases of this condition aiding in or being a cause of the production of the deformity, it must, I think, be very rarely a determining factor. I have never satisfied myself of its existence in any of the cases that have come

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under my notice, and I observe in Mr. Symington's excellent paper on flat-foot in the October number of the *Journal of Anatomy and Physiology* that Mr. Golding Bird failed to find it in any of his cases, which were 50 in number, although he always looked for it.

A twofold cause is mentioned by the late Professor Hueter (*Grundriss der Chirurgie*)—one a hyperpronation of the foot in infancy when, namely, the child begins to walk, coupled with and possibly caused by ligamentous relaxation. This latter view is combated by Von Meyer, who denies the relaxation of the calcaneo-scaphoid ligament, and attributes the deformity to a displacement of the astragalus due to an excess of its natural movements. Whether this opinion is founded on *post-mortem* examination or is merely the result of theoretic speculation I am unable to say. But whether it is or not, due weight must be attached to his opinion on the subject.

In the debate which took place last August at the International Medical Congress at Copenhagen on talipes and its treatment, the opinions of several authorities were distinctly in favour of the view—contrary to what is generally held and taught—that to original osseous malformation, rather than to muscular or ligamentous abnormality, must we look for explanation of these deformities. The opinion I then expressed was strongly opposed to this view, but I am free to admit that from what I then heard and have since ascertained, I should not, were the discussion to take place again, express myself quite so decidedly as I did.

If the original malformation theory holds good of the ordinary forms of talipes it should do so equally of pes planus, and a study of Mr. Symington's case tends greatly to strengthen this view. The signal failure that so frequently attends the mechanical treatment adopted in these cases also does so, as well as the inspection of the specimen which, through the kindness of our President, I am able to lay before the meeting.

It was, in truth, with feelings of no ordinary satisfaction that I obtained the opportunity of inspecting this singularly interesting specimen just alluded to—the only one of the kind, so far as I am aware, in this country, and which verifies to so great an extent the

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views I have held so long as to the ætiology of the defect. Professor Cunningham, too, who has carefully examined this specimen, agrees with me in the propriety of dismissing the idea that ligamentous relaxation would alone account for the changes here present, and the same remark may be made in reference to paralysis being an ætiological factor. The principal osseous abnormalities are those observed in the astragalus and os calcis. In the former of these it may be observed that the head of the bone is directed downwards instead of forwards, and its articular surface in contact, not with the scaphoid, but inferior calcaneo-scaphoid ligament; that on the upper and now anterior portion of the neck of the bone is an abnormal facet articulating with a somewhat hypertrophied scaphoid; that the distance between the posterior margin of the scaphoid and anterior edge of sustentaculum tali is nearly double what it is in the normal condition of things; that the sustentaculum tali, instead of forming a nearly horizontal base of support for the astragalus head, is directed obliquely downwards and forwards, the groove immediately posterior to it, as pointed out to me by Professor Cunningham, looking backwards instead of downwards. The head of the astragalus is enlarged, and the neck of the bone is lengthened on its upper or now anterior surface, whereas posteriorly its length is normal or nearly so. The body of the bone is apparently unaltered, its trochlear surface maintaining its normal relations.

If the views of Von Meyer and those who think with him were correct, that, namely, the deformity is due to displacement of the astragalus forwards and inwards, the body of the bone should partake of the displacement as well as the neck and head, the trochlear surface should be directed forwards, and a new articular facet formed, or an extension of the normal one backwards.

The interosseous ligament is evidently the means of keeping the body of the bone in its position and relieving the calcaneo-scaphoid ligament of pressure. That it did so in this case is obvious, inasmuch as its upper articular surface preserves its normal relations.

The changes in the position of the bone indicated by Von Meyer are in this case non-existent.

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My contention, therefore, is that we must look to something else besides relaxed ligaments and paralysed muscles as causing these remarkable changes, though I do not, of course, deny that they may not at times be present.

Having regard to the osseous deformities in this remarkable case, as well as the very similar state of things found in Mr. Symington's, it is not surprising that the routine line of practice already alluded to should, as a rule, prove so ineffective. This is especially obvious in those instances constituting, I believe, the majority of those cases in which there is, as may be here observed, an abnormal projecting angle or ridge separating the articular surface into two portions—one articulating with the scaphoid, and the other with the inferior calcaneo-scaphoid ligament. This projecting ridge forms an effectual barrier in preventing a restoration of the arch by any pressure made from without.

Case.—In April, 1884, a youth, aged fourteen, came under my care in the Richmond Hospital, in one of whose feet all the physical characters of flat-foot were well marked. He was a well-nourished, strongly-built, fresh-complexioned lad, whose health generally had always been excellent. He complained greatly of pain in the foot, especially about the medio-tarsal joint, after walking for a short distance or standing for even a very limited time, and consequently he was unable, as his relatives stated, to take sufficient exercise or join in the recreations of his companions. The case appeared to me to be in every way suitable for operative interference, the condition of things causing much trouble to the patient, and the case being one in which the arch of the foot could not be restored by any reasonable force or pressure from without. In order to render the parts to be operated on as perfectly aseptic as possible for several days—a process similar to that recommended by Professor Ogston of frequent washing with a (1–20) solution of carbolic acid, rubbing off all superfluous epidermis, and keeping the foot day and night carefully wrapped in several layers of moistened carbolised gauze, and applying immediately before the operation spirits of turpentine—I made an incision (under a carbolic spray strongly directed on the part) an inch and a half in length along the inner edge of the foot, the centre of which incision was the prominence caused by the head of the astragalus, though

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of this, I confess, I was not quite certain, until I exposed it, as to whether, namely, the tumour was formed by the head of the astragalus or tuberosity of the scaphoid. In some cases I have little doubt it is caused by the latter.

At the centre of the incision I made along the inner edge of the foot, I made another three-quarters of an inch in length, and at right angles to the first, and a little behind the situation of Chopart's joint, and dissected back the two small triangular flaps of skin for about half an inch. My object was, if possible, not to open the joint, but to remove a wedge-shaped piece of the enlarged head of the astragalus, which wedge should have its base below and apex above, and which, if removed, would enable the depressed portion of the tarsus to be elevated, and the arch restored. On taking out the wedge, which I did with an osteotome, I found that it was impossible to avoid implicating Chopart's joint, and accordingly a portion of bone was removed, which included a part of the abnormal facet which articulated with the scaphoid. I found then that by adducting and supinating the foot the arch was perfectly restored. Listerian dressings were then applied, but without drainage. In the healing of the wound there was no pus production, and the patient was afebrile throughout. On the wound being healed, the method I adopted for keeping the foot in a position of adduction was a simple one, and one to be recommended in these cases—namely, the application of a Dupuytren's splint applied as in a case of Pott's fracture of the fibula. In this case it was kept on for a fortnight, when a gypsum bandage was substituted for it. The result was most satisfactory, and an examination of the case, made in the sixth month after the operation, was such as to make me look forward with confidence to the good result being permanent, the patient informing me—and his statement was endorsed by his friends—that he was able to walk, run, and play about as well as any other healthy boy of his own age.

From the foregoing remarks the following propositions may be considered:—

(1.) That the theory of ligamentous relaxation being the chief factor in the production of flat-foot is erroneous, being in the majority of cases the result and not the cause of that deformity.

(2.) That elongation of the calcaneo-scaphoid ligament should not be mistaken, as it so often is, for relaxation of it.

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(3.) That the altered direction of the sustentaculum tali is a change that could not be directly or indirectly connected with either ligamentous relaxation or muscular paralysis.

(4.) That the osseous deformation, whether resulting from original malformation, or rickets, or other pathological change in genu valgum—a condition until quite recently believed to depend solely on ligamentous relaxation—furnishes an *à priori* argument in favour of the author's theory.

(5.) That the treatment of pes planus, in cases, at all events, where the deformity is irreducible, should be directed mainly to restoring the arch of the foot by operative interference with the misshapen astragalus.

(6.) That this is feasible without destruction of the medio-tarsal joint.

(7.) That the evidence in favour of muscular paralysis being an ætiological movement in the production of flat-foot is insufficient and unsatisfactory.

(8.) That deformity of the scaphoid, the result of disease, may also be looked upon as a cause of flat-foot.

(9.) That the appearances in the specimens noted by Mr. Symington and the author furnish evidence as to the probable connexion between flat-foot and original osseous malformation.

(10.) That after astragaloid osteotomy it is desirable to keep the foot, during the healing of the wound, in a state of supination, which can conveniently be effected by the application of a Dupuytren's splint, as used in fractured fibula.

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ON FLAT-FOOT TREATED BY OGSTON'S METHOD.

By KENDAL FRANKS, M.D., Dub.; F.R.C.S.;
Surgeon to the Adelaide Hospital, &c.

[Read in the Surgical Section, January 23, 1885.]

ALTHOUGH I have only one case of flat-foot or pes planus to report, in which I have employed Ogston's operation as a means of cure, I have not hesitated to bring it forward to-night, as I believe it will not be altogether devoid of interest to the members of the Surgical Section of the Academy. In the first place it will form an appropriate corollary to the paper read by Mr. Stokes, and in the second place it illustrates in this region the wide field of beneficent results obtainable by operative measures under the protective influence of aseptic surgery. Until quite recently the treatment of flat-foot by operation, except in the milder forms, was eminently unsatisfactory, and had been almost entirely abandoned. In most cases the treatment was only palliative, and even in this it most frequently failed. I need not now refer to the operative measures which were formerly in vogue, such as tenotomy, and operations which dealt only with the plantar fascia or the ligament of the sole of the foot, and which failed to bring about a cure of the deformity. My object is to bring under your notice the plan of dealing with these cases introduced by Prof. Ogston, of Aberdeen, in a paper read at the Medical Society of London in January of last year, and to contrast it with the very ingenious plan devised by Mr. Stokes, and which you have heard this evening.

In cases of flat-foot, we have to deal with a sinking downwards of the arch of the foot. It is very difficult to understand how this is in the first instance induced, but I am strongly inclined to

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acquiesce in the generally-received opinion that one of the primary factors, and indeed the most important one, is a relaxation of the usually strong ligaments which bind together the bones forming the arch, and chief among which is the calcaneo-scaphoid ligament. Granting this, the sequel is not hard to understand. The whole weight of the body resting on an arch, thus deprived of its most essential means of support, brings about readily those changes in the bones and joints which we find in a well-marked case of flat-foot. Many theories have from time to time been propounded to account for the relaxed condition of the soft parts, such as muscular paralysis, congenital relaxation, constitutional debility, and soforth, but the sum total of them is this, that we are as ignorant of the starting-point of the mischief as we are of the origin of evil itself. I think we can hardly accept the congenital theory, as in the majority of cases the deformity is not developed till puberty. By far the larger number of cases occur first in growing boys and girls, and it would be difficult to reconcile this with the idea that the disease was due to some congenital defect. This view of the case may have arisen from the fact that, as Prof. Ogston says, flat-foot is generally confounded with pes valgus—a congenital deformity whose site is in the ankle-joint, and which is also characterised by eversion of the foot, whilst in flat-foot the ankle-joint rarely participates. On this ground I cannot, therefore, accept the view that congenital deformity of the astragalus itself is the cause of the disease, nor would this explain why sometimes the joint between the scaphoid and internal cuneiform is also affected—so much so that in one case Prof. Ogston found it necessary to deal with this joint also. Now, in the foot we have an arch formed of several articulating bones, the arch form being mainly maintained by strong bands beneath, which hold the bones together, and this arch has to support the whole weight of the body. If from any cause the restraining bands become slack, the superincumbent pressure will cause the arch to yield gradually; and the joint where this pressure exerts its greatest influence is the astragalo-scaphoid articulation, the inner portion of Chopart's joint. As the arch sinks, what must happen in this joint? In

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the first place, the lower part of the joint must gape, as the bones here gradually separate, and there will be a corresponding pressure exercised between the upper surfaces of the articulating bones. This is all we find in recent cases, before changes in the bones themselves have had time to form. In such a case the deformity can be readily reduced by manipulation. As the patient continues to walk on the limb, a vicious cycle is induced. The great strain put on the already relaxed ligament makes it relax more and more, and the arch continues to descend, until it disappears altogether and we find the bones on the inside of the foot forming a straight line. The foot no longer rests on the heel and ball of the foot alone, but the head of the astragalus and the tubercle of the scaphoid also rest on the ground, forming then a well-marked prominence on the inner border of the foot. This is well illustrated by the impression which I took of my patient's foot previously to operation. The way in which the impression was taken was this: The soles of both feet were well rubbed with charcoal finely powdered, and she was then made to stand on two pieces of damped paper. When the impressions thus taken were dry, they were varnished. No. 1 shows the right foot, which was chiefly affected. We can see here how the inner border of the foot rests in its whole length on the ground. The remarkable prominence marks the position of the astragalo-scaphoid joint. The eversion of the foot was well-marked. I found I could to a great extent abolish the deformity by manipulation, when the patient was lying down. By grasping the prominal end of the foot and fixing the astragalus, whilst with other hand the distal half of the foot was drawn inwards, the fingers pressing the astragalo-scaphoid joint upwards, the arch of the foot was to a great extent restored. I could not completely restore it on account of the alterations which had taken place in the articulating surfaces, and to which I shall immediately refer. If, on the contrary, we remove the manual pressure from Chopart's joint, and evert the foot, the extreme deformity seen in the illustration at once reappeared. Now I maintain that were the condition caused by a congenital malformation of the head and neck of the astragalus, as Mr. Stokes

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believes, it would not be possible, by these very simple means, to make the deformity disappear and reappear at pleasure. And this experience is not confined to my case. It is true of all cases of flat-foot which have not lasted long enough to cause changes in the bones to any great extent. "So long as the hand maintains Chopart's joint in its proper position," says Prof. Ogston, "the flat-foot cannot be rendered evident." It was this fact which suggested to Prof. Ogston the proper method of dealing with such cases. "When observing the disease," he says, "and my failures in its treatment, I could not help being struck with the prominent part played in its production by Chopart's joint, and became convinced that, could any method be devised of restoring the joint to its normal position and rigidity, or even of causing bony anchylosis there, it would almost surely result in a cure. From observing that so long as the manipulating hand held Chopart's joint firmly reduced, so long the deformity was apparently cured, the inference was natural that any method of imitating from within the effect of this support from without, would have a good prospect of radically curing the disease. Anchylosis between the astragalus and the scaphoid could do no great harm, for there are so many other points at which the tarsus is movable, that probably the rigidity of this one joint would entail no inconvenience."

Let me now call attention for a few minutes to those more severe cases in which this reduction by manipulation is only partially successful, as in my own case, or in which it may be quite impossible. This, I have said, is due to secondary changes which take place in the articulating surfaces of the bones.

It is perfectly evident that if we were to straighten out the bony arch of a normal foot forcibly, we could only succeed, in the first place, by either dividing or stretching the ligaments supporting the arch. In the next place, the under-surface of an arch being of necessity shorter than the upper, if we attempt to flatten out the arch, the extremities of the arch surfaces maintaining their relative positions, this can only be effected by the formation of gaps in the lower arch surface, or by a shortening of the upper surface, or it may be occasioned by a combination of these two. Now, in flat-

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foot this gaping of the lower surface of the arch occurs first. It takes place sometimes at Chopart's joint alone, but occasionally the articulation between the scaphoid behind and the internal cuneiform in front becomes also relaxed. Hence, at this stage it is easy to see why the deformity can be abolished by pressing the articulating bones into their usual healthy position. On the other hand, it is obvious that the pressure on the upper surface of the arch due to the weight of the body resting on it, pushes these bones downwards, causing the articulating surfaces in their lower halves to become separated, and at the same time causing their upper halves to be abnormally pressed together. Such a condition could not long exist in any joint without bringing about alterations in the form of the articulating surfaces, but in the joints of patients as yet not fully developed (and it is at this period of life that flat-foot is mostly developed), the changes in the bones is more rapid, complete and marked. The abnormal pressure between the upper halves of these bones checks their development, or may induce absorption, whilst the diminished pressure in their lower halves causes hyperdevelopment or growth to take place, so that in process of time no gap or separation exists, the convex and concave appearance of the articular surfaces being completely lost. A similar condition is observed in cases of genu valgum. In MacEwen's valuable monograph on osteotomy he shows that in these cases the chief cause of the deformity is in the vast majority of cases due to an "internal curve" of the lower third of the femur. This causes an undue amount of pressure on the outer condyle, and a diminution of pressure on the inner. What is the result? By careful measurements he has found that in 84 per cent. of the cases of knock-knee "the internal condyle was abnormally elongated," the elongation varying from a quarter of an inch to an inch and a quarter, and my own experience has convinced me of the truth of these observations.

Well, then, in flat-foot we find similar changes taking place. The growth in the upper halves of the articulating surfaces is more or less suspended, or even a certain amount of absorption or atrophy may take place. In the lower halves we have an opposite

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condition. Prof. Ogston thus describes it:—"The increased growth takes place mainly in the astragalus, so that the joint is not found gaping below, but the caput tali is changed in shape, becomes somewhat square in form, and presents an abnormal ridge or projecting angle, dividing its articular facet into two portions, one articulating with the scaphoid, the other with the inferior calcaneo-scaphoid ligament. When this disposition of bone becomes very prominent, the flat-foot enters on its permanent stage, and the more marked the projecting angle becomes, the greater is the resistance offered to the reduction of the deformity. The angle ultimately projects so much that it locks on the scaphoid, and no reduction is possible until it be removed. The relaxation and subsequent alteration of shape in the bones of Chopart's joint are the key to the disease and its successful operative treatment."

The details of the operation recommended by Prof. Ogston were carried out accurately in the case on which I operated, and therefore, to avoid repetition, I shall relate them in connexion with the case:—

M. B., aged twenty-one, was admitted to the Adelaide Hospital, under my care, on 1st October, 1884, suffering from flat-foot. She was a strong, healthy-looking girl, somewhat anæmic, and suffering frequently from neuralgia. When thirteen years old she got a fall, after which she suffered from pain and slight swelling in the right foot when she walked. This pain was not very troublesome until about April last, when it became very constant. By occupation she is a shop assistant, which required her to be constantly on her feet. Though she had not given up work, she was obliged frequently to take rest. When she applied for admission to the hospital, she begged that any operation which would afford her relief might be done, as the pain was intolerable. On examination I found that the right foot rested in its whole length on the ground on the inner side, the astragalo-scaphoid joint forming a well-marked prominence, as shown in the illustration; the foot was markedly everted, and the great toe had lost its natural semi-extended position and was in a straight line with the sole of the foot. For three days previous to operation I adopted the precautionary measures, as advised by Prof. Ogston, in order to secure perfect asepticity of the parts. The foot was washed twice a day

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in a hot 1 in 20 solution of carbolic acid. The softened layers of thick epidermis were then well rubbed with pumice stone and a nail brush, and finally the foot was enveloped in a large Lister's dressing.

On the 16th of October the operation was performed. Ether having been administered, the foot was cleansed with a mixture of one part of oil of eucalyptus in three parts of turpentine, and subsequently with the 1 in 20 carbolic lotion, and a purified waterproof sheet was placed beneath the limb. Assisted by my colleagues, I operated as follows: An incision about 1½ inches long was made parallel to the sole, its centre corresponding to the position of the joint. In a healthy foot, Chopart's joint lies about an inch in front of the internal malleolus, but in flat-foot, owing to the abnormal shape of the astragalus, it lies an inch and a half in front of it, so that the incision, begun an inch from the malleolus, will have its middle point over the joint. This incision goes down straight to the bones and divides no important structure. This incision generally penetrates the capsular ligament, but if not another application of the knife will accomplish this. To gain free access to the joint, the attachment of the capsular ligament to the scaphoid was dissected off both upwards and downwards. By this means a T-shaped opening was made into the joint, and the articulating surfaces were then brought into view. The next step of the operation was to denude the bones. This was done with an ordinary stout chisel, half an inch broad and bevelled on one side. In denuding the scaphoid the bevelled side was turned towards the bone so as to preserve the concavity of its surface. The cartilage and sufficient bone to expose the cancellous structure were then pared off. In dealing with the astragalus I found that the lower half of its articulating surface projected forwards so much that it was impossible to completely rectify the deformity until I had removed this projecting portion. The rest of the head of the bone was denuded, the bevelled side of the chisel being turned away from the bone so as to preserve the naturally convex surface, as advised by Prof. Ogston. This is, to my mind, a superfluous precaution, for, as we are aiming at complete anchylosis and not at a moveable joint, it matters little what shape the opposing surfaces are, so long as they are accurately adapted to each other.

I then found that I could restore the arch of the foot perfectly. Our next object was to fix it in this position. The parts being firmly held *in situ* by my colleague Mr. Barton, I drilled two holes obliquely through the scaphoid—one above and one below—making

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them penetrate well into the head of the astragalus. Into these holes I gently hammered two ivory pegs, carefully prepared and rendered aseptic. Unfortunately, one broke whilst I was inserting it, so that the bones were only held by one. In any future operation of the kind, I purpose to abandon this pegging together of the bones, as I believe it is unnecessary, for we can keep the arch firmly fixed in its restored position by putting up the foot in an inverted position. We thus obviate the necessity of leaving a foreign body in the wound, which, though generally innocuous, may give rise to some trouble afterwards.

The edges of the wound were then brought together with catgut, and a few threads of this were inserted as a drain. The entire foot was enveloped in a Lister's dressing, and the whole surrounded with a plaster-of-Paris bandage. Some severe pain was experienced for the first couple of days, but the temperature during the whole after-treatment never rose above 99·2°. The first dressings were removed on the 21st Oct.—that is, the sixth day, when the wound was found perfectly healed. A second plaster splint was then applied, a pad being placed under the arch of the foot. The patient was kept in bed for three months, and on the 16th of this month was allowed up. She cannot yet walk comfortably on the foot, owing to the stiffness in the ankle caused by its long confinement in the plaster, but every day this is improving, and I expect very soon to discharge her able to walk without pain. On Monday last, the 19th, I took this charcoal impression of her foot as she stood on a damp piece of paper, resting her whole weight on the right foot, being supported only by the elbows. It shows a remarkable contrast to the impression taken before operation. Not only has the prominence due to the astragalus disappeared, but from the ball of the foot to the heel, the inner border of the foot has failed to leave any impression on the paper.

It may be said that this case is of too recent date to prove anything as to the value of the operation, and I confess that on this account I would have withheld it for the present, were it not that the subject was to be brought before the Academy by Mr. Stokes this evening. I thought that more would be gained in interest by bringing it forward in connexion with Mr. Stokes' case, than would be lost by bringing it forward prematurely. But in testimony to the value of this operation, we have more than one

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case to go upon. The results which Prof. Ogston has published speak for themselves. When he read his paper last January in London, he had operated seventeen times in ten patients—sixteen of the operations were successful. One patient alone expressed dissatisfaction with the result, as one of his feet ached him a little. "Yet he underwent a season of heavy harvest work after leaving the infirmary." In a few cases "the plantar arch was restored to perfection. But this was not generally the case. In all it was, however, much improved, and added to the patient's lightness of step, even when, to a surgical eye, traces of the deformity remained."

In conclusion, then, I believe that this operation will become established as the best means of dealing with those cases of flat-foot which do not yield to palliative treatment, because it removes the cause of the deformity itself. It would be difficult to understand how the arch of the foot could again sink, if bony anchylosis exist in the astragalo-scaphoid joint. In my opinion it must prove superior to Mr. Stokes' method of astragaloid osteotomy, not perhaps in its immediate results, but as a permanent cure. In flat-foot, let us remember, we have an arch yielding because a gap or fissure gradually forms between two bones forming that arch. The base of that gap is below, the apex above; if by manipulation we abolish this gap, the deformity in the arch disappears. Now, astragaloid osteotomy rectifies the deformity by cutting in one of the bones a second gap, or removing a wedge, the base of which is also below and the apex above. By approximating the edges of this second gap the arch is restored—temporarily I believe. For how, I ask, does this remove the cause? How does it prevent the astragalo-scaphoid joint from gaping still further, or from becoming more and more distorted? Evidently it does not. Chopart's joint is capable of an extreme amount of deformity, as is seen in those severe cases of flat-foot when the body rests on the inverted arch of the foot alone, the heel and the ball of the foot being so raised off the ground that the whole foot has been compared in shape to a canoe. There is nothing in the nature of astragaloid osteotomy to prevent the weight of the body continuing to act on the relaxed structures, which allow the astragalo-scaphoid joint to

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yield, and therefore nothing to prevent the re-sinking of this joint to take place. As in flat-foot itself, so in a foot on which astragalo-osteotomy has been performed for its cure, the deformity may go on forming for months or years before it has become sufficiently marked to give rise to painful symptoms. It would therefore be premature to draw conclusions as to the ultimate results of astragaloid osteotomy, from the very interesting case brought forward to-night; but on theoretical grounds, and those are the only grounds yet open to us, I think we must conclude that, however brilliant in its primary results, astragaloid osteotomy does not give that future promise of success which is offered to us by Ogston's operation.

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IVORY EXOSTOSIS OF THE AUDITORY MEATUS.

By ARTHUR H. BENSON, F.R.C.S.;

Ophthalmic and Aural Surgeon to the City of Dublin Hospital; Assistant-Surgeon to St. Mark's Ophthalmic Hospital.

[Read in the Surgical Section, February 20, 1885.]

It is not many years since the first case of successful operation for the removal of ivory exostosis of the meatus was recorded, and though since then several such have, from time to time, been placed on record, the case which I wish to bring before the Academy of Medicine possesses some points of interest which, perhaps, justify its publication.

The terms "exostosis" and "hyperostosis" are ones which are rather indefinitely used by different authors, and this has led to confusion. Cassells[a] is at considerable pains to distinguish between these two forms; the term "exostosis" he confines to *new growths* of bone in the meatus, usually ossified granulations, tolerably soft, tender to the touch, usually pedunculated, growing from the posterior wall, and only to be found arising at the outer half of the external auditory meatus, having their origin in the periosteum of the mastoid, close to the meatus, and usually single.

The "hyperostoses" are *hyperplasms*, are never pedunculated, are usually of intense hardness, not tender to the touch, may grow from any portion of the osseous canal, and are usually multiple. The following case would, therefore, belong, according to the above definition, to the class of "hyperostoses," but I have preferred to follow the more usual, if not the more scientific, nomenclature:—

Case.—Mr. N., aged thirty-three, of full habit of body and an indefinitely gouty and rheumatic descent, was sent to me by Dr. Allen, of Middleton.

[a] Trans. Int. Med. Congress. 1881. Vol. III.

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History.—He stated that formerly he used to bathe much in the river, but that he gave it up many years ago because it affected his ears, and he suffered from ear-ache, which he sought to relieve by applying laudanum and sweet oil, but that for the last three or four years he had not had any trouble of that kind. Recently he had applied to Dr. Allen, in consequence of deafness in the right ear, caused by an accumulation of cerumen in the meatus. On removing this with some difficulty, an exostosis was observed, and he was recommended to consult me, which he did in September, 1883.

Stat. Præs.—I found in the right ear three bony bosses protruding into, and partially blocking up, the lumen of the meatus externus. Two of these were situated close to the membrana tympani, and grew from the anterior and posterior walls of the meatus. Between them a tolerably wide slit existed, through which the membrane could be seen to be healthy. The third boss, which was also the largest, was growing from the anterior wall of the canal, and a considerable distance external to the others. It was conical in shape, and extended into the meatus so as to render full examination of the deeper growths difficult. Around and internal to this there was a considerable collection of cerumen and epithelial *debris*.

It seemed that the deafness was entirely caused by the accumulation of cerumen, &c., behind this bony prominence, for when the collection was removed by syringing the hearing was perfectly normal again.

This most external tumour was covered with pale, smooth skin, was not tender to the touch, appeared of ivory hardness, and was perfectly immovable. In the left ear a somewhat similar condition prevailed, but to a much less degree.

The diagnosis was plain, and the question of treatment at once presented itself. The choice lay between some operation for the removal of the growth, and purely palliative measures, as removing the cerumen from time to time; for few will now-a-days place much reliance upon local applications of nitrate of silver, or iodine tincture, or yet upon blisters to the mastoid, or iodide of potassium internally—all of which have, from time to time, been used and praised by high authorities. The obvious disadvantage of adopting the palliative course was that sooner or later the passage would become too small to permit the removal of the cerumen, &c., and operation would then be imperative, whilst the growth being bigger

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and the passage smaller, the difficulties of the case would be greatly increased.

On the other hand, to undertake so serious an operation as the removal of an ivory exostosis before it became absolutely necessary to do so, seemed to me to be open to objections also. I therefore determined to try the effect of "electrolysis." I judged that by applying the current with a needle to the tumour in a number of places, I could so destroy the vitality of its covering as to produce a superficial necrosis, and, perhaps, in this way, remove the growth sufficiently.

Treatment.—A ten cell Leclanché's battery was accordingly brought into play, and a gold needle attached to the negative pole was thrust into the skin, covering the exostosis, the circuit being completed by making contact over the mastoid process with a pad attached to the positive pole.

In order to get at the internal aspect of the growth, I had a needle bent into a hook, which I was able to pass behind the apex of the cone, and stick into its internal surface. The pain caused by the application was considerable, but not excessive, and no pain or inflammatory reaction followed the *séance*. The application lasted five or six minutes, and after the removal of the needle the meatus was dusted with boracic acid in powder. Fourteen or fifteen such applications were made on consecutive days, and the whole top of the tumour was thus riddled with needle holes, and denuded of its soft parts. The patient was then sent down to the country, and I asked Dr. Allen to keep a look out for whatever might come from the ear. Six weeks later Dr. Allen sent me up a shell of bone, the top of the exostosis which had necrosed, and which he had removed when syringing the ear. It was evident, therefore, that the treatment had, so far, succeeded.

On January 24, 1884, five months after his first visit, he again consulted me. The exostosis had grown but very little, if at all, since I last saw him. The hearing was perfect, and the ear was not tender. He was, however, anxious that something further should be done, as the cerumen still collected on and behind the growth, and I gave him a choice of another term of electrolysis, or the more rapid and radical removal by the aid of a dental engine. He chose the latter.

Operation.—Mr. Arthur Baker kindly assisted me at the operation, which was done in his consulting room. Sitting in the dental chair, ether was administered, and a vulcanite speculum, which

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was cut short for the occasion, was inserted as a guard to the soft parts.

The idea was to perforate the base of the tumour with a drill, and then with a saw or file[a] to cut upwards and downwards from the central hole. This I found impossible to do, owing to the extreme density of the tumour, its sloping side, and the difficulty which I experienced in working the drill in the ear. After some unsuccessful attempts to make an impression on the side of the base, Mr. A. Baker suggested that it might be more easy to use the saw from the top. The soft parts over the exostosis were removed by a few turns of the rose, and the saw (which Mr. Baker showed) being attached to the engine, a groove was, with much difficulty, cut by Mr. Baker in the base of the tumour; into this a small chisel was inserted, and the bony apex of the exostosis which I show, was, by a slight blow of the hammer, removed. There was a very considerable amount of bleeding during the operation, which, however, soon ceased, and the ear was plugged with boracic acid powder. The greatest possible difficulty was experienced in working in the ear with the instruments intended for the teeth, as they were not long enough to reach in without blocking up the view of the parts, and rendering it necessary to stop and syringe out the meatus every few seconds.

Had it not been for Mr. Baker's skill in working the engine, I do not think the operation would have succeeded, for he did most of the latter part of the work. The operation lasted two full hours, and we were both fairly exhausted when it was over. No bad symptoms followed, and in a fortnight the ear was looking quite healed; the hearing, which for a few days was defective from the swelling, &c., was normal, and he returned to the country. Since then I have seen him from time to time, last on January 20, 1885, just a year after the operation. There was not the slightest trace of an exostosis in the old position, only a slight scar marking the place it had occupied. The exostoses near the membrana tympani were, perhaps, somewhat larger, but hearing was then, and had since been, perfect. The exostoses in his other ear were somewhat more prominent, and larger than when last seen.

Into the question of ætiology I shall not enter, so many and various are the theories which have been from time to time

[a] The saw is more like a screw; it is used on the flat, and cuts with a steady motion without any backward and forward, or sawing motion whatever.

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advanced. My patient had never had rheumatism, syphilis, or gout; he had bathed in fresh, but not in salt water, and in his opinion it was the bathing which first started the ear trouble. This would agree with Field's ideas, but in his case the agent was salt water. It seems probable that very many different causes may produce the periosteal changes which give rise to ivory exostosis.

There has of late years been a steady and ever-increasing flow of opinion in favour of early operations in many classes of cases.

I am aware that, in operating in the case above described, I violated the rules laid down by most competent authorities, and I would, therefore, especially wish to obtain the opinion of the members of this Academy on two questions:

I. What is the proper time to operate on an exostosis?

II. What is the best method to adopt?

Most writers[a] lay it down as a canon that the operation is not to be performed until "total deafness has occurred in both ears," unless in the case of otorrhœa, where an exit must be provided for the discharges. It seems, however, reasonable, in view of the result of the case I have just described, to say that the earlier operation may, under some conditions, be the best. If the operation can be done with tolerable safety, and with but little fear of the tumour growing again, I think that the earlier it is performed the better—before the parts get much changed, whilst still it is possible to know the exact condition of, and see the exact position of, the membrane, and before the patient has had to endure the dreadful misery of gradually getting deaf, with all its isolation and despondency.

As to the best method of operating, each case must be considered according to its peculiar features. In electrolysis we possess an agent capable of doing good service in certain cases, and with patients too nervous to undergo the more formidable operation it would probably be borne well, and, combined with the application of acids to the denuded surface, might prove curative. It is, however, slow, and rather painful.

[a] Politzer, Cassells, Barr, Burnett, &c.

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Of the gouge and hammer alone I shall say nothing; the method seems to deserve nothing but condemnation.

That which we adopted in this case seems about as likely to succeed as any method, and has the advantage of almost absolute safety. It is much more easy for a drill to go astray in its course, and penetrate some vital part, than for the so-called saw to get out of the proper direction; and by making the cut only through, say, three-fourths of the tumour, and separating the remainder with a chisel and hammer, almost all danger of wounding the membrana tympani, or other parts in the middle ear, is avoided. It would be advisable, however, to get the saw considerably longer in the shank than the ordinary dentist's instrument, as with it the difficulty of illumination is very great.

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THE OPERATIVE TREATMENT OF ENTROPIUM AND TRICHIASIS.

By JOHN B. STORY, F.R.C.S.;
Surgeon to St. Mark's Ophthalmic Hospital, Dublin.

[Read in the Surgical Section, February 20, 1885.]

In a paper published in the *Ophthalmic Review* of February, 1883, I gave a description of the admirable operation proposed by Dianoux, of Nantes, for the cure of trichiasis, and expressed my conviction that the operation there described was, without exception, the best of all the then known methods of correcting the malposition of the ciliary border, and alleviating the unhappy condition to which the sufferers from trachoma (granular ophthalmia) are so frequently reduced.

While I have nothing to add to the description formerly given of Dianoux's operation, it is natural to expect that the experience of the past two years will have led me to modify in some degree the views which I held at a time when the operation was, in these countries at least, a complete novelty. At that time I had myself observed only the results of four operations, all perfectly successful, and although my subsequent experience contains an almost unbroken list of similarly perfect successes, it has compelled me to qualify and correct some important propositions which I hazarded upon the strength of the results in those first four cases.

I stated (*loco cit.*) that the raw under-surface of the ciliary flap is found after Dianoux's operation to adhere with great rapidity to the upper cutaneous surface of the skin flap which is pulled beneath it at its two extremities, so much so that after three days it becomes impossible to pass a probe between the two surfaces, and after a few weeks even the detection of an operation cicatrix becomes a matter of extreme difficulty.

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Even at the time I made this observation it was to me a very incomprehensible thing how the external cuticle could adhere in this firm union to the under-surface of the ciliary flap, and I was not surprised when my friend, Dr. Arthur Benson, informed me some time afterwards that he had been able, many weeks after a successful operation, to pass a probe along the submerged epithelial surface without resistance, and without the appearance of any hæmorrhage to indicate the solution of vital union between the two surfaces. It was at that time his opinion that vital union never took place, and my subsequent observations render the truth of this opinion very highly probable. I have seen cases, one of which I have exhibited this evening, in which the little fistulous openings have remained unchanged for upwards of a year. It is, therefore, to my mind not at all improbable that the reason I failed to detect any fistulous opening in my early cases was my delicacy in handling the probe. In every case when I have tried to insert such an instrument as a blunt-pointed pin, I have succeeded in doing so without producing either pain or hæmorrhage.

I have also seen reason to modify my views as to the non-irritating properties of the external cuticle when applied to the surface of the cornea, and as to the pseudo-mucous character that the skin so situated takes on. It is but rarely that the skin acts as an irritant, but I have in the past two years seen at least two cases in which the small hairs on the transplanted skin acted as a source of corneal irritation, and I have seen many cases in which the appearance of the skin remained after several months markedly distinct from that of the neighbouring conjunctiva. In three of these latter cases I observed fairly long cutaneous hairs, but they did not appear to cause any trouble in the corneal tissue.

The only serious objection then to Dianoux's operation consists in the presence of cutaneous hairs, which, as I have stated, occasionally act as mechanical irritants to the corneal tissue. After I became aware of this danger, I always made a careful examination of the palpebral skin before operating, and if I found any considerable quantity of hair in it, or found the hairs, though not numerous, well developed individually, I rejected Dianoux's

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for some one of the other numerous procedures which have been adopted to cure either entropium or trichiasis. While I was in this uncertain state I received from Dr. Van Millingen a Report of his Ophthalmic Hospital at Constantinople, containing the description of a method of operating for trichiasis, which he had arrived at by modifying the well-known Arlt-Jæsche operation. This Report was published in 1881, some two years prior to my paper in the *Ophthalmic Review*, and a year prior to Dianoux's article in the *Annales d'Oculistique*. Van Millingen only applied his operation to cases of trichiasis without incurvation of the tarsus.

The modification consists in transplanting a small strip of skin, or, still better, of rabbit's mucous membrane, to the space between the cilia and the conjunctiva. This is done after the first act of the Arlt-Jæsche operation is completed, and the strip of skin or membrane is left in its new position without applying any sutures; it may be covered with a piece of goldbeater's skin, and secured by a bandage. It is well, Van Millingen writes, to effect a broad gap at the intermarginal space before transplanting the strip of skin, and this he does by drawing up the skin of the upper eyelid against the forehead by means of sutures; these may be allowed to remain in until the skin has taken, which generally occurs in twenty-four hours. Van Millingen takes the skin from the upper eyelid if the skin there is found to be hairless after examination with a magnifying glass—if not, he uses rabbit's mucous membrane; and had, in 1881, successfully used that from the under-lip of rabbits for this purpose.

This modification, described by Van Millingen, it may be observed, contains two separate and distinct procedures—one the transplantation of palpebral skin (without pedicle), and the other the transplantation of rabbit's mucous membrane in a similar way.

The first of these procedures is only practically distinguished from Dianoux's method by the greater probability it unhappily offers for sloughing of the transplanted skin, and I have consequently never adopted it. Van Millingen is not the only oculist who has advocated a proceeding of this description. Dr. E. Jæsche proposed in the *Klin. Monats-Blatt.* for January, 1881, to

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cover the raw surface beneath the ciliary flap with epithelium from the arm. Dr. Waldhauer also, at the Eisenach meeting in 1882, advocated a somewhat similar method (*vide Ophthalmic Review*, Vol. I., p. 412).

The second commends itself at once to every scientific surgeon as likely, if successful, to produce a more perfect result than any other method that has yet been practised in this country, but to carry it out as described by Van Millingen entails all the trouble and annoyance of procuring a license under the Vivisection Act, and exposing both the hospital and the surgeon to the odium that would be excited thereby among a large section of the benevolent public. No doubt the end could be equally well attained by first slaughtering the unhappy rodent, which would render any actual vivisection unnecessary, but not to mention the utterly needless cruelty involved, it seems a somewhat extravagant proceeding to sacrifice a whole rabbit for the sake of a few millimetres of perfectly superfluous epithelium.

A far more important consideration, however, must occur to anyone who devotes any thought to the objects to be attained by the operation. If rabbits' mucous membrane can attain these objects, will not human mucous membrane do so in a still higher degree? The objects are to afford support to the ciliary border and so prevent its reinversion, and to do that by a tissue which will produce no more corneal irritation than would be produced by the healthy palpebral conjunctiva. Now we have in the buccal mucous membrane a tissue which is eminently adapted for fulfilling these proposed ends, and I have accordingly taken my strip of mucous membrane from the patient's own under-lip, and can now, after some ten months' experience, confidently recommend this operation to any surgeon who desires to please both himself and his patients by trichiasis or entropium operations.

I have notes of nineteen cases performed in St. Mark's Ophthalmic Hospital since April 23rd, 1884, of which at least eight were done by myself, the others either by Mr. Arthur Benson or the resident surgeon. All these operations were successful so far as a cure of the deformity is concerned, even including one in

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which nearly the whole of the strip of mucous membrane sloughed; but it is probable that in this latter case a relapse will occur sooner or later, necessitating the performance of a secondary operation.

The operation, as I now perform it, is as follows. It is done with or without the inhalation of an anæsthetic, at the option of the patient:—

Knapp's speculum being applied to the upper lid to prevent hæmorrhage, the intermarginal incision of Flarer is made from end to end of the lid, and the tissue of the orbicularis, &c., is freely dissected up off the tarsus. The lid clamp is then removed, in order to allow of the restoration of vascular tonus and permanent stoppage of bleeding before the strip of mucous membrane is transplanted upon the space left bare beneath the ciliary margin. The clamp is in the meantime applied to the lower lip of the patient so as to restrain bleeding, while a thin strip of mucous membrane is dissected off the buccal surface. The strip should be of the length of the intermarginal incision, or slightly longer, and of a breadth of from 2 to 4 mm. according to the necessities of the case. Care should be taken to remove all the fat from the mucous membrane before fixing it in its new situation, and a useful plan to prevent much—or any—fat adhering to it as it is raised from the submucous tissue is to effect this dissection in a partial manner by a succession of transfixions by means of an ordinary iridectomy knife, this being done before the end of the strip is taken up in a forceps and lifted up bodily from its attachments. For this suggestion, as well as for the application of Knapp's speculum to the under-lip, I am indebted to my friend, Dr. Arthur Benson. All remaining fat being carefully removed from the little strip of mucous membrane, it is then attached by means of a suture at both its ends (and occasionally a suture at its centre) to the ends of the intermarginal incision, and the buccal bleeding controlled by a few minutes' application of pressure.

The only drawback to the method of dissecting the strip of mucous membrane by transfixion is, that occasionally the whole band may be cut in two from awkwardness on the part of the

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surgeon or the patient. If this occurs an additional suture is necessary, but the effect of the operation is not in the least lessened by such an accident.

The operation which I have described differs but in the smallest degree from that advocated by Van Millingen in the Report quoted above. Indeed, it is hard to imagine how the idea of taking mucous membrane from the patient's mouth could have failed to present itself to the Constantinople oculist when he travelled in search of epithelium from the eyelid of a man to the under-lip of a rabbit. I was accordingly not at all surprised to find in the Report of Van Millingen's Hospital for 1882 that he had himself practised and advocated this very proceeding before it had occurred to me.[a]

The only points worth noting that differ in the operation as practised at St. Mark's Ophthalmic Hospital from the method originally proposed by Van Millingen are that the palpebral skin is not drawn up to the forehead by sutures; that, on the other hand, sutures are used to fix the strip of mucous membrane in its new situation, and that the operation is used for the cure of entropium proper (when the tarsus is curved in upon itself), as well as of trichiasis, to which cases its use is absolutely restricted by Van Millingen. Now, I have never yet seen any necessity for raising the palpebral skin by attaching it to the forehead. Indeed, if sutures are to be employed elsewhere than in the transplanted mucous membrane, my inclination would be to use them to draw the tarsus down (by attaching it to the lower lid) rather than to draw the skin up. Sutures seem to me, however, necessary in order to fix the little strip of mucous membrane in its place. It might, no doubt, lie steadily without their presence, but it is decidedly better to employ them and obviate the risk of the tiny strip of tissue being disarranged by some casual movement of the patients.

For cases of incurvation of the tarsus, Van Millingen employs Streatfield's operation, or some modification thereof. Now, this

[a] Dr. Van Millingen's Report was published last year, and I received a copy through his kindness in January this year, 1885.

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grooving of the tarsus has always seemed to me a piece of unnecessary surgery. If there were no hairs at the ciliary margin to rub upon the corneal epithelium it would not matter to anyone how much incurved his tarsus happened to be, provided always that the closure of the lids were not interfered with. If this view be correct, it is manifest that Van Millingen's operation should be applicable equally to cases of entropium and trichiasis, and my experience has fully borne this out. Out of the nineteen cases successfully operated on in the last ten or eleven months in St. Mark's Ophthalmic Hospital, the deformity was caused by actual incurvation of the tarsus in eleven. In five of these eleven cases I performed the operations myself, and as I have always held somewhat peculiar views upon this subject, I have always been careful to note whether the tarsus was or was not incurved before operating. These five cases may, then, be regarded as test cases, and from them the conclusion may be drawn that the grooving of the tarsus is quite unnecessary for the cure of entropium caused by incurvation of the tarsus, the simple transplantation of a strip of mucous membrane into the intermarginal space being quite sufficient to effect a cure of this deformity as well as of trichiasis.

The operation here described conforms in all essentials to the requisites for a good operation which I laid down in 1883 (*Ophthalmic Review*, Feb., 1883). No skin or other tissue is unnecessarily removed, no cicatrices are left to subsequently alter the results, a firm support is given by the strip of mucous membrane to the ciliary border which effectually prevents its reinversion, and, lastly, no difficulty whatsoever is put in the way of the performance of any secondary operation in the improbable event of the first being unsuccessful.

The dressing which I am in the habit of applying after the operation consists in an ointment of iodoform (10 grains to the ounce of vaseline) spread on lint, which is placed directly upon the wound. Over this is laid a piece of cotton wool soaked in cold water, which is kept in its place by a light bandage. The wool is kept thoroughly moist by the nurse in charge of the case, and I find that this plan prevents the lint sticking to the wound and so

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rendering it impossible to remove the dressings without disturbing the delicate strip of transplanted tissue.

In conclusion, I would lay stress upon the fact that the operation proposed is equally applicable to cases of pure entropium as to those of simple trichiasis.

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1. COMPLETE EXCISION OF THE CLAVICLE FOR TUMOUR (OSTEO-SARCOMA).

2. PARTIAL EXCISION OF THE SAME FOR NECROSIS.

By W. I. WHEELER, F.R.C.S.;

Surgeon and Lecturer on Clinical and Operative Surgery to the City of Dublin Hospital.

[Read in the Surgical Section, March 20, 1885.]

Entire or even partial excision of the clavicle—the girdle bone of the shoulder—is an unusual operation, and one that surgeons are not often called on to perform. Having regard to its rarity, I have decided to bring before this Academy the histories and results of two cases—one of complete, the other of partial, excision of the clavicle.

Unfortunately in representing the first case I am only enabled to produce the cast, the specimen of it having been destroyed:—

Case I.—M. B., a native of Wexford, aged forty-three, was admitted into the City of Dublin Hospital under my care in 1874, suffering from a tumour of considerable size on the left collar bone, extending over its entire length, with the exception of about three-quarters of an inch of the sternal end and an inch and about a half of the acromial extremity. It was dense and firmly attached to the bone, occupying the anterior inferior and part of the posterior inferior triangles of the neck. It hung down over the inferior or pectoral edge of the bone, and had the appearance of being part of the clavicle itself. Within the patient's recollection no hurt had in that region been occasioned, except a slight blow which he received by having a door forcibly pushed against him. He was a spare, thin man, his height about 5 feet 7 inches, weighing about 10½ stones. His countenance wore an anxious and careworn expression. He complained of some pain down his arm and in his

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hand. The latter was sometimes œdematous, resulting, probably, from the pressure of the tumour on the subclavian vein.

After due deliberation and consultation with my colleague, Mr. Tufnell, I determined to remove the clavicle, and consequently, the patient having been fully anæsthetised, I made a concave incision downwards along the clavicle from the sternal articulation on the opposite side to the extremity of the acromial process, and I reflected both up and down the integument and fascia. The clavicular origin of the sterno-cleido-mastoid was also divided, along with the other necessary muscular structures. I next opened the acromio-clavicular articulation, and lifting up the bone by means of a lion forceps, I detached the costo-coraco-clavicular ligament and the subclavian muscle. Consequent upon the upward extent of the tumour I found it necessary to make a vertical incision extending into the posterior superior triangle of the neck, and by careful dissection and manipulation separating each attachment with the greatest care, so as to avoid the large vessels directly beneath. Both the subclavian vein and artery were easily recognisable, as also the cords of the brachial plexus. Fourteen ligatures were applied—two on the external jugular vein. The supra-scapular artery (which was much enlarged) was also tied. A few small vessels were twisted. All hæmorrhage having been controlled, the wound was brought together by interrupted sutures. A suitable pad producing necessary pressure was placed upon it. The patient's arm was bandaged to his side, and he was replaced in bed. No appreciable amount of suppuration ensued; free drainage was maintained; the wound, however, closed slowly. The man did not leave the hospital for seven weeks after the operation, by which time the incision had completely healed.

I saw this patient three months subsequently at Kingstown. He was pale and anæmic. He suffered from cough, and presented the appearance of one who ere long would develop phthisis. He was then able to use his arm and shoulder with tolerable facility, and but for the condition of his general health he could have earned a livelihood.

To my agreeable surprise I was accosted last May in Sackville-street by him. He had just returned from America, and was looking well, having gained both flesh and strength. I then had an opportunity of seeing the result of my operation. To an ordinary

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observer the man had every power as though the clavicle had not been removed, but when exercising a forward motion of his arm the scapula, instead of moving circularly, glided on the ribs. I saw him once since then, when he promised that he would appear here in the event of my requesting him to do so.

The second case is not, comparatively speaking, possessed of so much interest. The operation consisted of the almost entire removal of a necrosed clavicle. It must, therefore, be correctly classified under partial excision:—

Case II.—This patient, a female, also a native of Wexford, is represented, by her history, as having been suddenly seized with pain in her left collar bone. The pain was acute enough to deprive her of sleep for weeks. It resulted in a large abscess, which eventually burst.

When the patient was admitted there were three sinuses leading down to the diseased bone. Having determined to remove the source of irritation, I made an incision over the clavicle, as in the case just recorded, and having separated the attachments of the subclavian muscle, costo-coraco-clavicular ligament, and a portion of the clavicular origin of the sterno-cleido-mastoid muscle, I (with a cutting forceps) divided the bone at the sternal end close to the articulation and at the external side about one inch from the acromial articulation. This patient progressed rapidly, and is now living in New Ross. There has been no sign of regeneration as far as I can learn. She has easy motion of her shoulder and arm.

The bone I removed is in the Museum of the Royal College of Surgeons.

My reasons may be questioned for not endeavouring to preserve the periosteum, that thus a chance for regeneration might be given—a result which has been known to occur in similar instances. Those reasons were twofold. In the man's case I deemed it inexpedient, my desire being to remove the bone in its entirety and not to leave remaining any portion either of it or of its covering. It was from the latter that the tumour had undoubtedly sprung. In the female's case the periosteum was destroyed, except over a very small portion of the sternal and acromial end of the bone which remained.

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In treating of excision of the clavicle many surgical writers fail adequately to emphasise the magnitude of such procedure. Explanation for this may not unnaturally be found in the surmise that those recorders have neither performed the operation themselves, nor possibly have they even seen it performed by others. Truly, Mr. Erichsen well describes it as one of the most hazardous operations in surgery. The operator is in close proximity to the subclavian vein and artery, also to the pleura, being only separated from it by the thoracico-cervical septum. He is close to the phrenic nerve, the common carotid artery, and, if operating upon the left side, he is near to the thoracic duct, which in Mott's cited case was exposed, the operation being for an osteo-sarcoma; but although generally looked upon and quoted as a complete excision, it was not such, since, in the published account of the operation, it is related that the acromial end of the bone was not interfered with, and the autopsy made years afterwards proved that the portion left measured one and three-quarter inches. Yet four hours were occupied in performing this operation, and forty ligatures were applied for the purpose of arresting hæmorrhage.

The necessity for excision of the clavicle, either partial or complete, appears to arise in the following cases:—Necrosis, compound fracture, tumour, gunshot injury, and in dislocation, for which the sternal end of the bone has been removed by Davie, of Bungay. M'Crearry, of Kentucky, appears to have been the first who removed the clavicle in its entirety. Although Remmers has been spoken of as performing complete excision in 1732, the operation really was only partial removal.

Statistics of extirpation of the clavicle have been published by O. Heyfilder, the late Dr. Otis, and Professor Agnew; but in my opinion the most reliable are those by Ashhurst, since many of those quoted by Otis and Agnew were only partial excisions.

Ashhurst gives 28 complete excisions, of which only 6 proved fatal, making a mortality of less than 22 per cent.; but to this conclusion I object, inasmuch as in one of the cases he accredits with recovery—that of Dr. Curtis—the disease had returned within two months after the operation.

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Of the 28 cases, 7 were for necrosis, 8 for caries, 2 for cancer, 4 osteo-sarcoma, 1 malignant disease, 1 enchondroma, 2 gunshot, 1 injury, 1 osteitis, 1 tumour. Of those that died, 1 enchondroma, 2 gunshot, 1 necrosis, 1 caries, 1 osteo-sarcoma.

Of Professor Agnew's 34 cited cases, 10 were for cancer, 5 for necrosis, 5 for osteo-sarcoma, 6 for malignant disease, 2 for shot injury, 1 for exostosis, 2 for tumour, 1 for injury, 2 unknown.

Of the partial excisions, including such formidable cases as Mott's and Barlett's, there are 74 recorded, of which 59 recovered and 10 died, the result in 5 being unknown—the mortality of the known cases, therefore, was but 14·5 per cent.

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RESECTION OF THE HUMERUS AT THE SHOULDER-JOINT.

By W. I. WHEELER, F.R.C.S.;
Ex-President and Member of Council, Royal College of Surgeons, Ireland;
Surgeon to the City of Dublin Hospital.

[Read in the Surgical Section, March 20, 1885.]

The fact that in civil practice cases are comparatively so rarely encountered necessitating the operation of resection of the humerus at the scapulo-humeral joint, induces me to record the histories and results of the three most recent instances in which I performed this operation. The portions of bone removed in two of these cases have already been shown at meetings in the Royal College of Surgeons in Ireland. Now I exhibit both the patient upon whom I last operated and also the diseased structures removed by that operation. The records upon this subject in civil practice are indeed limited. In the fourth volume of the "International Encyclopædia of Surgery" the accomplished writer states that only in a single instance during his experience was he obliged to resort to this particular operation.

The first case whose treatment I shall detail was admitted under my care into the City of Dublin Hospital in October, 1881:—

Case I.—A female, aged thirty-seven years, having disease of her right shoulder-joint. The record of her case states that two years previously, when milking, she was knocked down by a kick, and her shoulder severely hurt; it was greatly swollen after the accident, and various remedies were applied to reduce existing inflammation and swelling, which, however, never completely disappeared; in about a year after the first injury she received a blow on the same shoulder; the swelling increased almost equally in extent to that resulting from the original injury, and within a year

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from this period an abscess formed, which, having broken, was discharging at the time of her admission to hospital.

There was no difficulty in determining the nature and extent of the disease. A probe could with facility be passed through the opening before referred to, whereby the extent of the carious condition was ascertainable, engaging both the head of the bone and the rim of the glenoid cavity. The operation of resection having been decided upon, I made an incision, commencing a little to the outer side of the coracoid process, and cut downwards and outwards for about five inches between the fibres of the deltoid muscle, and in the line of the bicipital groove. The long head of the biceps muscle was held to one side by an assistant. The capsular ligament was freely divided; the tuberosities of the humerus were rotated into the wound, the muscles attached to the greater divided, and the subscapularis, inserted into the smaller, severed; the head of the bone was next protruded, and the diseased portion, which extended to a little below the anatomical neck, was removed by means of Mr. Butcher's saw. The diseased rim of the glenoid cavity was cut off with a cutting forceps suitable to the purpose. After hæmorrhage (which was very trivial) had been arrested, a drainage tube was inserted, one end passing through an opening made at the posterior and inner side of the arm. The wound was brought together by points of interrupted suture. The arm was supported on a pad somewhat resembling Stromeyer's cushion, and water-dressing was applied over the wound.

This treatment resulted in a rapid cure, the patient leaving the hospital completely recovered in December, 1881, five weeks after the operation had been performed, the wound having healed nine days previously to her discharge. I had an opportunity of seeing this woman five months after she left hospital. She had a most useful arm, and the overhand motions were not very deficient, considering the short time that elapsed since she had been operated on. I am since informed that all the motions have steadily improved.

Case II.—A male, aged forty, admitted in March, 1882, suffering from caries of his left shoulder-joint. The record of his case attributes the disease to the swing bar of a plough falling on his shoulder, which produced acute synovitis, chronic thickening of the synovial capsule, degeneration of the cartilage covering the head of the humerus and the glenoid cavity, and, subsequently, carious disease. On examining this patient a slight fulness was

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perceptible around the articulation, and on manipulation a distinct crepitus was easily perceivable both by touch and sound. On passing the humerus towards the glenoid cavity there was grating, plainly showing that the cartilages had been destroyed. I operated on this patient in a similar manner to that already described, except that the long tendon of the biceps muscle did not remain intact; it appears to have given way at its origin before the operation; in this case only one side of the glenoid cavity required to be gouged. A portion of the head of the humerus was separated from the shaft of the bone, and was quite loose. The disease extended to about an inch below the tuberosities, which were removed. This patient did not heal rapidly; a sinus remained in the site of the incision which led down to a small portion of the humerus, which was denuded and carious. I enlarged this opening and removed the very small but diseased piece of bone; he now rapidly healed, and was discharged from hospital in September, 1882. A month after the wound had completely closed. I have not had an opportunity of seeing this patient since, but my friend, Mr. Pratt, informs me that he has a useful arm, that he is earning his livelihood as a labourer, and that his overhand motions are not defective.

The third case to record is that of the patient brought before this meeting. Not three months since he was discharged from hospital; his history is as follows:—

Case III.—T. D., aged thirty, a strong and, apparently, healthy farmer, was admitted into the City of Dublin Hospital suffering from disease of his left shoulder-joint in September, 1884, recommended by my friend, Dr. Charles Stoney.

In August, 1883, this patient had hurt his shoulder by raising with it the wheel of his cart, which had stuck in a bog. Shortly afterwards his shoulder got stiff, so that he was unable to raise his hand to his head; it was also greatly swollen, but without being painful. Before admission his shoulder-joint had been twice aspirated with an interval of five days.

On the first occasion about five fluid ounces were removed; on the second occasion about a pint. On his admission the effusion was considerable, and the head of the bone was pushed downwards.

On October 23rd, the patient being anæsthetised, I proceeded to

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resect his shoulder-joint by a single straight incision, as in the other cases. The capsule was greatly thickened, and on opening it there was a gush of synovial fluid, mixed with blood, and considerably over a pint in quantity. I dissected away the entire capsule, which was very adherent. I removed the head of the bone and a portion of the inferior and external part of the glenoid cavity, also the tip of the acromion process which had been fractured and was carious. The long tendon of the biceps was preserved; all hæmorrhage was controlled; the wound was brought together by means of interrupted suture, a drainage tube having been previously introduced. The arm was placed on a Stromeyer's cushion, and the patient replaced in bed. A few hours after the operation a slight oozing of blood took place, which increased, thereby rendering it necessary to re-open the incision. The hæmorrhage came from a small vessel situated on the posterior and external surface of the bone, and bound down so tightly to the latter as to make it impossible satisfactorily to secure it by ligature; I therefore arrested this bleeding by a compress steeped in Ruspini's styptic.

For the first four days succeeding the operation this man suffered from retention of urine. The shoulder wound healed kindly, and he was ordered to be up on the 8th of November; but on the evening of the 7th he got an attack of inflammation in the veins of his right leg (phlebitis), from which he was not convalescent until the 10th of December. He was discharged from hospital on the 24th. He has excellent use of his arm, including some overhand motion, which he tells me has been daily improving since he left the hospital, not quite three months ago.

Excision of the shoulder-joint for disease is not so commonly performed as is excision of other articulations, for the simple reason that disease of the articular structures of this joint is not frequent, and when it does occur it often terminates in ankylosis. Again, it is not very uncommon to find that supposed disease of the shoulder-joint is merely caries of the coracoid or acromion process, and that the osseous structures forming the joint are unimpaired; consequently, the removal of such diseased parts will be sufficient; indeed, small sequestra, even in the articulation, may be successfully removed without resecting this joint. But there are other causes, apart from disease, which may demand resection

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of the scapulo-humeral joint, such as gunshot wounds, old and painful as well as compound dislocations, tumours of the head of the bone not cancerous, compound and comminuted fracture of the articulation.

It makes, however, very little difference for what cause we may have to perform excision of this joint, so long as the treatment be adopted which will prove most beneficial to the patient, and which will endow the hand (to which the other joints of the upper extremity are secondary) with the widest range of usefulness.

With regard to the incisions for this operation several have been advocated—the T, the V, the inverted ⅂, as well as the longitudinal incision through the deltoid muscle, which, in my opinion, is the one calculated to afford the best results, and which is therefore the one I practise. There are several movements in the shoulder-joint—as rotation, abduction, elevation, adduction, and antero-posterior movements—and if the deltoid be cut across, the power of abduction and elevation must be lost, but if its fibres are merely divided the probability is that their movements may be retained. With regard to the saving of the long tendon of the biceps, I strongly advocate that, when possible, this should be preserved. I am aware that in many cases of disease it is destroyed and good results have ensued, but I have seen more useful arms and more extensive motion follow when it has remained intact. I have not found it necessary to continue making the counter-openings for drainage. The position of the arm is undoubtedly away from the side and somewhat forward; this favours the contraction of the wound, and the bonds effecting repair are shorter.

Excision of the shoulder-joint is a favourable operation; but for comprehensive statistics we are limited to the operations performed for gunshot injuries.

Hodges collected 50 cases of excision of the head of the humerus for disease; 8 died and 42 recovered. In 17 out of the 50 the glenoid cavity was operated on, but in 7 out of the 8 fatal cases this cavity required operative procedure.

Out of 115 excisions for disease in civil practice but 21 deaths are recorded—a mortality of 18·2 per cent.; and in military surgery

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35·4 per cent.; but there being many circumstances in a campaign which would tend to increase this mortality, it behoves the surgeon to select that period which is most favourable for the operation—the first stage, or that within twenty-four hours, being more favourable than the second stage, which is from the second day until suppuration is established; and the third stage, which is after the occurrence of suppuration, being the next most favourable time to the first.

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CONDYLOTOMY BY THE OSTEOTOME FOR THE TREATMENT OF KNOCK-KNEE.

By ROBERT L. SWAN, F.R.C.S.;
Surgeon to the Orthopædic Hospital.

[Read in the Surgical Section, March 20, 1885.]

I SHOULD not have taken up the attention of the Academy by any references to this familiar subject, were it not represented to me that such a record as I had to furnish was of some value in showing the safety of the operation above-named. If, however, the matter is uninteresting, because well known to the members of the Academy, I shall not fall into the error of exasperating by an unnecessary prolixity.

It is within the recollection of almost every surgeon here what the teachings and experiences of even a few years ago impressed on the professional mind in reference to the knee-joint as a surgical region. Any encroachment on its cavity was regarded as fraught with peril, and the existence of a painful abnormality which threatened each step with anguish, and rendered existence unendurable, alone justified its exploration. It was my friend, Mr. Reeves, of the London Hospital, who, about eight years ago, introduced the method of detaching the condyle of the femur by the chisel for the rectification of genu valgum. He named it extra-articular condylotomy. This term, I admit, held out an attraction for me; for the rest, I could easily see that the principle of restoration of the limb to a straight line by a displacement of the inner condyle was correct, and that such an object could in all cases be accomplished. It was not until many early attempts to separate the condyle almost into the joint that I perceived the futility of such efforts, and, emboldened by success, I laid aside those precautions. It cannot be denied that other means, well known

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and practised by many surgeons here, have been found extremely successful; but, while in no way depreciating those methods, the deductions obtained from a somewhat large experience in the mode which I have almost invariably adopted, justify the assertion that it is safe and certain.

I will briefly narrate the steps of the operation. Having Esmarched the limb, a line of considerable obliquity is mapped out, through which the condyle must be separated so as to allow it to glide easily from its position. Having done so, the soft tissues are cut transversely, and the bone reached in front of the internal condyloid ridge. Having placed the osteotome firmly against the bone, a direction in the line previously indicated is followed, either by the assistance of the mallet, or by the force exerted by the hand alone, as the osseous structures vary in density. Having reached a point corresponding with the assumed surface of the inter-condyloid fossa, the osteotome is withdrawn, and a second cut is made anteriorly, and a third posteriorly, in the same line. Using the osteotome at this time as a lever, it is usually ascertained that the condyle is detached. It only remains then to seize the limb and forcibly bring it to a straight position.

The subsequent dressing is of much importance. The site of operation should be carefully padded, so as to exercise a pressure on the parts and secure their immobility, and a plaster-of-Paris case applied before the removal of the Esmarch ligature, to prevent extravasation into the tissues and its evil sequences. After eight days the plaster case may be removed, and it will be found that the joint is quite free from effusion. The small external wound, whose lips were loosely held by a catgut suture, generally is seen to exude a sero-sanious fluid, but this wound soon heals. Passive motion may from this time be resorted to daily. In an accompanying table are the names of 68 individuals, on whom I have operated during the past four years. I have had the advice and assistance of my friends and colleagues, Professor Hamilton, Dr. Robert M'Donnell, and Dr. Cranny, in most of them. I will briefly summarise this table of the 68 cases:—Only 7 had unilateral deformity, so that the number of knees operated on were in all

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129. The ages ranged from 19 years to 3½, as follows:—One at 19, one at 18, two at 17, two at 16, two at 15, two at 14, two at 13, four at 12, one at 10, five at 9, seven at 8, six at 7, five at 6, nine at 5, eleven at 4, and five at 3½ years.

Nineteen of those cases had both knees operated on simultaneously—namely, one at 18, one at 17, one at 16, one at 10, one at 9, two at 8, two at 7, five at 5, and four at 4 years old.

In one of those cases, aged five years, I separated the inner condyle of one femur, and the external of the other, for opposite deformities. The result was excellent.

In one case, M. Wilkinson, aged sixteen, the temperature rose on the eleventh day. I found that suppuration had occurred under the quadriceps. I maintained a free drainage through the original wound, and injected daily with a 1 in 60 solution of carbolic acid. A good recovery ensued.

In the case of John Whitty, aged five, a synovitis of the joint supervened. This assumed a purulent character. I made a free incision at the internal aspect of the joint, giving exit to a quantity of synovia mixed with pus. After a fortnight's careful dressing the wound had closed. He recovered with a perfect use of the joint.

The foregoing are the only two instances in which any unfavourable circumstance occurred; and, indeed, I may make the assertion that the remainder endured no uneasiness during the convalescence.

The density of the bone varies considerably in different individuals. While it is generally easy in young subjects to detach the condyle by the osteotome forcibly impelled by the pressure of the hand, in other instances the tissue is of stony hardness. It is, therefore, prudent not to proceed to operate in such a case without being provided with a mallet.

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Table of Cases of Genu Valgum.

	Age		Age
James Hopkins	4	J. Whitty	5
John Richards	7	F. M'Guinness	4
Ed. Hamilton	7	D. Kennedy	3½
Robert Hamilton	5	Hugh Hodgson	4½
Thos. Fottrell	3½	Mary Smith	7
Robert Keenan	8	Rose Edwards	17
Lizzie Henderson	13	B. Nolan	9
Peter Knox	7	Catherine Moroney	5
Edwin James	3	Mary Kane	6
Mary Kelly	6	Ann Smith	3
Thomas Ovens	5½	Peter Black	9
K. Rynd	8	Mary Farelly	14
M. Martin	5	Peter M'Laughlin	4
C. M'Donald	8	Olivia Henderson	7
J. Elston	15	George Freer	9
D. Phillips	12½	Thomas Toomey	8
T. Dempsey	14	William Doyle	13
C. Bryan	16	Michael Martin	18
J. Flanagan	5	Joseph Handley	9
T. Burke	8	Kate Kenny	10
J. Wyber	17	Samuel Kelly	4½
H. M'Nulty	4	Robert Sharp	9
J. Cawl	6	Sarah Cassin	6
J. Keegan	8	Georgina Power	5
F. Ellworth	7	Nelly Cope	13
E. Heney	5	Anne Graham	6
T. Malcolm	8	David Jennings	11
M. Halligan	4	Harriet Redmond	4
M. Brophy	12	Matilda Wilkinson	16
E. Healy	4	Alexander Nolan	5
Chris. Ryan	14	Bridget Manders	13
J. Roberts	3½	Thomas M'Mahon	15
J. Murphy	3	William Carroll	6
M. O'Shea	19	L. R.	13
James Phelan	5	E. R.	4

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SOME CASES IN KNEE SURGERY.

By JOHN K. BARTON, F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Surgical Section, April 24, 1885.]

It has occurred to me that by grouping together in one paper a number of cases, having this only in common, that they are all concerned with the *same region*, while they differ in other respects, we shall have an interesting illustration of the surgery of that region, while at the same time we shall have the variety which we meet with in actual practice, and which, while free from the monotony of a number of cases of a precisely similar kind, yet will have enough in common to admit of some general conclusions and interesting suggestions regarding treatment. The region which I have chosen to-night is the knee, and the cases I have collected illustrating the surgery of this important part are six in number, and are as follows:—

No. 1. Excision of the Joint for progressive disease.

No. 2. Excision for Deformity, the result of past disease.

No. 3. Amputation through the Joint.

No. 4. Chronic Enlargement of the Bursa Patella removed by operation.

No. 5. Extraction of a loose Cartilage from the Joint.

No. 6. Extraction of a Needle from the immediate vicinity of the Joint.

I will briefly relate the leading features of these cases, and as I do so draw attention to the points in each, which seem to me of special importance.

Case I.—*Excision of the Knee-joint for Progressive Disease.*—John Boon, a lad of fourteen years of age, was admitted under my care into the Adelaide Hospital, on the 16th of May, 1884. His right

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knee-joint was the seat of disease, which had at that time lasted for fully two years. The joint measured two inches more in circumference than the left one, and the limb was three-quarters of an inch shorter than the sound one. The character of the swelling was that of Brodie's pulpy degeneration; a deep ragged opening existed just above the external condyle of the femur, through which the probe passed in for a distance of two inches, but did not encounter any bare bone.

The history of the case was, that two years previously to his admission the joint began to enlarge, and became very painful, which was attributed to a severe blow which he received shortly before right on the patella. He had been in the City of Dublin Hospital under Mr. Fitzgibbon's care, who treated the joint by firing. The boy was benefited by this treatment, and left the hospital; but the disease seems to have advanced after he returned home, and when probably he used the limb much more freely than it could bear with safety.

The diagnosis which I arrived at was chronic synovitis and ulceration of the cartilages, with probable ostitis in the immediate vicinity of the articular surfaces; and the treatment decided upon was *excision*.

The operation was performed on the 26th of May by a semilunar incision under the spray, and with full antiseptic precautions. I also used Esmarch's elastic bandage. The cartilages were extensively ulcerated, and where not removed by ulceration they were loose, white, and necrotic.

The apparatus for supporting the limb during the after-treatment which I employed was one which I first saw used by you, Mr. President, in a similar case. I have made some changes in it which seemed to me improvements, and which I will refer to presently.

The question of an apparatus for excision of the knee, now that we use antiseptic dressings, resolves itself into a choice between one which will hold the limb steady while we apply the dressings *inside it*—in which case the apparatus must branch out round the joint—*or* one which will lie so close to the limb as to allow us to apply the dressing *outside it*—in other words, to include it in the folds of the dressing, without, at the same time, so separating the dressing from the limb as to admit the air under it.

The strong strap wire which I employed in this and the next case is designed to fulfil the latter of these two ideas. It is fixed to a circular steel hoop, which fits round the chest; from this it passes

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down below the buttock, popliteal space, and leg, until, arriving at the heel, it passes along the sole of the foot, ending in a hook, which serves to suspend the limb from a Salter's swing. Care must be taken to mould it accurately to the limb before the operation. It should lie very closely, and if this is done it acts exceedingly well, as will be seen by the records of these cases.

The case went on for a fortnight perfectly, so that I anticipated a more than usually rapid recovery, when a change took place; the wound, from being quite aseptic and nearly healed, began to suppurate and open; the surrounding parts became red, swollen, and brawny, and this extended up the thigh as high as the trochanter; evidently inflammatory action had extended from the cut surfaces along the periosteum of the femur. This change was due, I believe, to two causes—first, that the boy moved his limb about, and thus excited inflammation in the cut surface of bone; and secondly, to the fact that at this time the antiseptic dressings no longer protected the wound, as they had done up to this. Both of them—viz., the motion and the destruction of our dressings—were due to the strange conduct of the boy, who seemed to have lost whatever of sense he had, and to have become an unaccountable maniac. He passed his urine and fæces in the bed and over the dressings, soaking them several times a day, and nothing that our ingenuity could devise would prevent him pursuing this filthy, and, in his case, destructive conduct. He had no diarrhœa, his appetite was natural, his bowels regular, but he seemed to take a morose pleasure in forcing us to take off all the dressings, and change everything in his bed. As might be expected erysipelas followed, and this further complicated the case. The summer months thus passed without firm union being obtained. At last, however, inflammation subsided, and simultaneously the boy's conduct completely changed; and from having been this filthy little savage, he became one of the best and most obedient boys in the hospital. The limb now, a year after operation, measures two inches less in length than the other, is perfectly strong, and bears Jack with safety and rapidity. [He is here to-night for examination.]

Case II.—*Excision of Knee for Deformity, the Result of Disease.*—Ethel Reilly, aged six, a delicate-looking little girl, was admitted into hospital in November, 1884, on account of lameness, the result of disease of the right knee-joint. The limb was found to be flexed to a right angle at the knee, and the head of the tibia

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was found to be displaced backwards. The knee had been the seat of active inflammation, the result of an injury received two years previously, which inflammation had so far destroyed the joint as to allow the head of the tibia to be drawn backwards by the hamstring muscles. The joint measured one inch and a quarter more in circumference than the other, but there was no pain, heat, or tenderness of the joint. When an attempt is made to straighten the limb the hamstring tendons oppose this being done, but adhesions in the joint itself seem also to prevent it.

The child was lame from the flexed state of the joint, not from disease; and the question at once presented itself, how shall the limb be restored? To place the child under chloroform, and then forcibly break down adhesions, and, if necessary, divide opposing tendons, seemed the shortest and quickest way as well as the best, as preserving the full length of the limb, and requiring no cutting operation. But to this a conclusive objection existed in the fact that the head of the tibia had passed backwards from its normal position. Any attempt to straighten the limb by forcibly drawing it down must inevitably have separated the upper epiphysis of the tibia, leaving the upper articular portion fixed in its abnormal situation behind the condyles of the femur, while the shaft would have come forwards—even had I first divided the hamstring tendons. I felt convinced that force applied to the shaft of the tibia to draw it forwards must separate it at its upper epiphysis. Resection of the damaged joint was, under these circumstances, the proper method of restoring the limb. This was accordingly performed upon the 18th of November last. The limb was brought into a straight position, and supported by the brass rod splint, same as was used in the last case. The wound was dressed with sero-sublimate dressings and healed soundly, the deeper parts uniting by first intention, the superficial by granulation. The child took measles in January, which delayed the progress of the case, but is now (March) quite well, and able to support herself on the excised knee, and to raise it off the bed when lying on her back. [This patient is here to be examined by members.] The limb in which the knee has been excised measures one inch and three-quarters less in length than the other; the flat of the foot comes naturally on the ground, and there is firm union. I think the result justifies the opinion that excision in this case was preferable to an attempt to forcibly straighten the limb.

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Case III.—*Amputation through the Knee-joint.*—This is a very rare operation, and for a very simple reason; to render it suitable in any particular case we require a combination of circumstances which are rarely found together:—First, the articular surface of the femur must be free from disease; this excludes all joint diseases. Secondly, there must be ample sound skin for the formation of a long flap; this excludes cases in which there has been extensive injury about the joint. Thirdly, the disease must extend up to the knee to require the operation, but must not involve the femur at all to render it allowable; thus the only cases in which we can properly resort to amputation through the joint are those in which there may be disease limited to the head of the tibia; or as in the following case, in which there was no disease at all, but in which the limb from the knee down was a useless appendage and encumbrance from paralysis:—Thomasina Wheelan, eleven years old, was admitted to the Adelaide Hospital in April, 1884, from the Cripples' Home, Bray, where she had been for about a year. She had suffered from infantile paralysis, which had deprived her of all power over the left leg from the knee down. She could move the thigh, and consequently the muscles of that region were well developed, and the limb there well grown, but there was a marked contrast in the limb below the knee; the muscles were atrophied and flabby, the skin coarse, purple, and cold, and that the bones had not grown to their full size for her age was evident by comparing the leg with the right one. The leg being helpless from the knee was not only incapable of supporting the body, but was much in the way, and besides was a cause of much pain. The circulation in the limb was feeble, and consequently in cold weather painful spots formed on the livid skin and ulcerated, and were with much difficulty induced to heal. It was evident that this limb was not only much in the way, but was a source of pain and debility; and further, that the child, who now got about on crutches, might, if supplied with an artificial limb instead of the useless living one, run about without crutch or stick.

The rare combination of conditions which are required to render amputation through the knee-joint allowable were here present; so upon the 22nd of April, 1884, I performed that operation as follows:—A long anterior flap was cut, the incision for which began at the prominence of the inner condyle, and was carried down through skin and fascia, fully two inches below the tubercle

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of the tibia, where it crossed the front of the limb, the corners being rounded, and ascended to the outer condyle. The flap thus formed was raised, including the patella; the lateral ligaments were divided, and then a short posterior flap of skin, not more than a quarter the length of the anterior, was cut, and the operation completed by the vessel and nerves being divided. The cartilage of the femur was thin but healthy; it was not touched; the patella was also left, and the flaps met easily.

The healing process was slow; a very small part of the flap sloughed, and when this separated, the flap contracted to some extent, exposing the cartilage-covered condyles of the femur to a slight degree. I feared the cartilages would perish and have to come away, but they did not—they remained. The closing of the wound was completed by the granulating process, and a useful stump results. [This child is also here for inspection.]

The chief advantages afforded by this operation appear to be—1st, the preservation of the full length of the femur; 2nd, the avoidance of all risks which follow the section of bone; 3rd, the broad and naturally expanded surface of bone, which forms the stump, and hence the peculiarly firm support afforded by it.

I am inclined now to think that my case would have been better without the retention of the patella, inasmuch as the flap would have come more easily and neatly over the condyles had it not been there; and probably I should have avoided the loss of the piece which sloughed, and thus delayed the healing. Had I another case of this kind, I would dissect out the patella, but I think this case shows that the cartilages, when healthy, may be left without any danger of their becoming necrotic.

Case IV.—*Chronic Enlargement of the Bursa Patellæ.*—A thin, hardy-looking countryman, seventy-three years of age, came up from Enfield, Co. Meath, to me, to ask if anything could be done to remove a tumour from the front of his right knee, which proved, upon examination, to be an enlarged bursa patellæ. It was the size of a large orange, but of a more oval shape, measuring 6½ inches in its long diameter from above downwards. The cast [exhibited] shows the size and shape of the tumour. Fluctuation was evident, but the nature of the contained fluid was by no means clear. It gave him no pain, but was much in his way, and as it

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was continuing to increase in size, he was most anxious that something should be done for its removal. The history he gave of this tumour was that, as age advanced, he found his back weak and stiff, and so when he wanted to stoop down for anything he got into the habit of always kneeling on his right knee. He found the bursa enlarging, but as it gave him no pain he still continued to kneel upon it. About six months ago he hurt it one day, and since this it has grown much more rapidly, and has become painful when pressed upon.

Taking into consideration the great thickness of the integument and of the sac of the bursa, as evidenced by the feel the tumour gave to the fingers, and also the size of the tumour, I came to the conclusion that the treatment by blistering would be inefficient in this case, and that it would be much better to remove a thin eliptic piece of the front of the skin and of the sac, empty out the contents, and obtain union between the flaps laid down on the back wall of the bursa.

The old man was put on a preparatory tonic treatment as he had a weak intermitting pulse, and upon Nov. 18 the operation was performed. A long eliptic piece of skin was removed from the front of the swelling, six inches long, and not more than half an inch broad at its widest part. The sac was then opened, and was found to be quite filled, not with fluid, but with a granular lymph resembling sago, which was firmly adherent to the interior of the sac. This was all carefully removed, some of it requiring to be scraped away. When this was done the walls of the bursa, now empty, were evenly laid down, a drainage-tube was inserted at each end, and the cut edges were held closely together by a continuous suture.

The operation had been done under carbolic spray. The wound was now dressed with pieces of old linen, which had been soaked in 1 per cent. solution of corrosive sublimate in serum of horse's blood. Five or six thick layers of this were laid over the wound; over this the usual carbolic gauze dressing was laid, and firmly bandaged. A splint was then placed behind the knee to ensure rest, and the limb bandaged to it.

The highest temperature was on the evening of the third day, when it marked 99·8°, from which it fell to normal in two days. There was no pain, and no sign of staining through the dressings, which consequently were not disturbed at all until the 23rd—that is, five days after the operation, when the wound was found

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quite aseptic and nearly healed throughout. The drainage-tubes were removed.

He left hospital, the wound perfectly healed, and with full use of his limb, on December 9th.

Cases of chronic enlargement of the bursa patellæ are usually, as in this case, of inflammatory origin, and, being rare, we may not be quite certain at first when we meet such a case whether it is better to treat it by blistering, in the hope of causing the absorption of its contents, or by operation. My case shows the advantage of the latter, and of the easiest and best method of operating. It is also interesting, being the first case in which we used the sero-sublimate dressing; and although the dressing had, after five days, a most fœtid smell, yet the wound was perfectly aseptic. There was no suppuration, although the bursa was the reverse of healthy, being converted by inflammation into a sac filled with granular lymph, hypertrophied and thickened. The painless course of the case, and the quick way in which union took place, appeared to me to place the new sero-sublimate dressing far before carbolic gauze—an opinion which, now that we have had ample opportunities of testing its efficacy and improving its preparation, has been fully sustained. In sero-sublimate 1 per cent. solution we have an antiseptic of the greatest efficacy. No doubt a further improvement may be effected, but in the meantime we have the very satisfactory certainty that we can use corrosive sublimate of this strength with safety, and that we have in it a first-rate antiseptic.

Case V.—*Loose Cartilage in the Knee.*—Patrick Wallis, thirty-eight, a farm labourer from Parsonstown, sought my advice at the Adelaide Hospital last February, for a pain in his right knee, which, upon examination, I found was caused by a loose cartilage which slipped from under the tendon of the quadriceps extensor to the outside, and then slipped back again, but did not appear to go down into the lower part of the joint.

He gave the following account of the origin of this disease:—He had frequently been wet to the skin, and was "rheumatic." Twelve months ago he for the first time had a bad aching pain in the right knee; this pain was rendered worse by walking, and

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specially annoyed him when going up stairs. The pain was most felt at the inner side of the joint.

Seven months ago he felt, for the first time, something loose in the joint; it seemed to him like a pea, which slipped about above the patella; its usual situation was above the inner condyle, but when disturbed by any sudden motion of the limb it moved to the outside. He not only felt the motion of this body, but could hear a sharp click when it was suddenly displaced.

Finding himself unable to work, and losing his sleep at night from the constant pain, which returned each night, and seemed to come like the dart so common in disease of the joint, he applied to Dr. Stoney, of Parsonstown, who found the loose cartilage, and explained the cause of his sufferings to him, but declined to operate, telling him that in the country these things could not be done, as if "anything happened him" he would be accused of killing him; but that in town they (the surgeons) did not care what people said of them, so that they would do it for him. Acting on the advice then given him, the poor man walked to Dublin, not being able to afford the train, and made his way to the Adelaide Hospital. His knee, as may be supposed, was not improved by a three days' march, nor his appearance by a black eye, which he had received in some lodging to which he resorted the first night he arrived in town.

A bath, food, and rest, improved his condition; and upon the 3rd of March, without any fear of man before me, but with the greatest confidence in the protection afforded to me by antiseptic surgery, I operated. By a free incision on the outside of the extensor tendon, I opened the joint, and removed the loose cartilage, which I now show. I put in no drainage-tube, but to prevent oozing of blood into the joint or between the cut surfaces I waited until all bleeding had ceased, and then bound up the limb *very tightly*, finishing by a bandage from the toes up, as my bandaging round the joint and wound was so tight as to render this advisable. My dressing was sero-sublimate gauze (1 per cent.). The temperature chart shows that on the evening of the operation 101° was reached, which next morning fell to 99·4°. On the evening of the 5th—that is, two days after operation, it was again 101°, next morning 99°, and never rose after this above 100°; on the 8th was *normal*, and continued so.

Considering that the joint which I had thus opened was not a healthy one, I thought it better to give calomel and opium during the five days, until normal temperature had been reached. The

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prescription was half a grain of opium and one grain of calomel, in pill, every six hours for first twenty-four hours, and then every eight hours, and during the last ten days every twelve hours; when the temperature was normal it was stopped. During this treatment the dressing was not removed at all; there was no sign of discharge; no pain, except on pressure over the wound. Upon the 9th, that is, six days after operation, it was dressed for the first time, when the wound was found united; no effusion into joint, or pain. A week after this Pat Waller left hospital, well satisfied with his visit to Dublin. He considered his knee perfectly fit to bear him all the way by road to Parsonstown, and was rather displeased when I urged him to go by train, and sent a porter with him to see him off, and pay his fare.

My sixth and last case is one which is constantly occurring, and yet as each such case differs from all others, while at the same time most serious results may follow, its brief narration may not be uninteresting:—

A healthy young girl, a housemaid, happened, on the 27th of February last, to have a needle stuck into the front of her dress, which needle fell out and caught in her skirt. She knelt down on the stairs to scrub them, and at once felt the needle run into her right knee. She instantly attempted to draw it out by the thread which was in the eye; in doing this the needle broke, as she supposed, for upon examining the needle which she had drawn out, she found it was broken in two; the eye end with the thread was in her hand, the pointed end was nowhere to be seen. She could not be certain that it was in her knee, but she thought it was. She walked a distance of a mile that afternoon, was lame, and suffered pain all the way. That night she slept but little, her knee being the seat of pain. Next day she was sent to me, and upon examination I found the spot where the needle had entered half way between the apex of the patella and the tubercle of the tibia, and just at the inner edge of the ligament. The point of entrance was marked by a minute black dot; pressure upon this spot gave pain, as also did motion of the joint. From the presence of these signs I inferred that the point of the needle was lodged in the tissues, and should be removed. I kept her in the hospital, directed her to lie in bed, with a poultice on the front of the knee, and a splint behind it; and on the following morning I

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proceeded to attempt the extraction of the broken point of the needle lodged, as I believed, in the ligament of the patella.

The method I adopted was, first, to render the operation bloodless by means of Esmarch's bandage; then to make an incision an inch long over the inner edge of the ligamentum patellæ, passing through the spot where the needle had entered. In most cases the needle lies obliquely, and expecting it to be so, I thought I should touch it in part of its length, but nothing could be felt or seen, although the help afforded by the elastic bandage was very great indeed. I continued to dissect down, and the wound was fully half an inch deep before I felt a something which encouraged me, and which I found to be a minute black spot. Seizing this with a catch forceps I drew out the needle, which lay at right angles to the axis of the limb, straight in towards the joint and firmly held in the fibres of the ligament.

Two practical questions occur to me from this case—1st, as to the diagnosis; and 2nd, the treatment.

The grounds upon which I concluded that the needle was there were localised pain on pressure, and uneasiness in movement; and I think these two will rarely deceive us. As to the treatment, very great difference of opinion exists amongst surgeons as to the necessity or advisability of operating in such cases, influenced very much, I think, by their views of the danger of leaving the needle there, and also the danger of the operation for its removal; and probably also a fear that after they had undertaken an operation they might not be able to find the needle. This happened to me some time ago, in a case similar to the one I have now related. I did not open the joint, and the patient has felt no more trouble from the needle.

In another case in which I was consulted last summer, a boy had a large needle broken off just above the joint. I strongly recommended an operation, thinking that the needle had not penetrated the joint as yet, but if not extracted would do so. But the parents followed the advice of their family doctor, who advised against any operation; and so far the boy is none the worse, for I understand he is going about as usual and thinks nothing of the needle.

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In spite of these and such cases, I venture to think that the surgeon, when he has satisfied himself that a needle, or a portion of a needle, is lodged in the immediate vicinity of the knee, should at once undertake an operation for its removal, and that such removal may with tolerable certainty be effected; while the bugbear of destructive inflammation of the joint should no longer be permitted to interfere, being effectually banished by careful attention to antiseptic precautions.

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THE TREATMENT OF STRICTURE BY INTERNAL URETHROTOMY.

By WILLIAM THORNLEY STOKER, M.D.;
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Surgeon to the Richmond Hospital, and to Swift's Hospital.

[Read in the Surgical Section, May 22, 1885.]

The observations which I have to make on the subject of stricture of the urethra will be narrowed within close limits, as my object is chiefly the advocacy, in suitable cases, of internal urethrotomy; and I will refer to other methods of treatment only so far as may be necessary to throw light upon that with which I propose specially to deal.

In 1871, when I was appointed a surgeon to the City of Dublin Hospital, and for some years afterwards, by far the most usual treatment in this city for the immediate relief of stricture by internal operation was that devised by Perrève, modified by Holt and others, and variously termed "immediate dilatation," "divulsion," or "bursting," according to the view taken by particular surgeons as to the mode in which the instrument acted; a latitude of opinion apparently justified by the cases mentioned by Mr. Richardson in his review of Sir H. Thompson's book in the *Dublin Journal of Medical Science* of August, 1870. In some of these cases, which had occurred in the practice of different surgeons and in which death from other causes followed the operation of immediate dilatation, the mucous membrane was found to have been ruptured, while in others it remained untorn. At that time internal urethrotomy was but little practised in the Dublin hospitals, and met with only a modified acceptance in London, for Sir H. Thompson, now one of its strongest advocates, was by no means so general in his commendation of it as he has of late

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become; but during the period which has elapsed since then it has firmly asserted itself to the exclusion of the other procedure. The change has been a gradual one, and, on that account perhaps, the better established; and although in Ireland we have been somewhat slow in adopting the cutting operation, we have now come to value it as much as could be desired. The reason why we adhered to immediate dilatation may, perhaps, be found in the revulsion of feeling with which surgeons welcomed any proceeding less risky than the earlier forms of urethrotomy, such as that of Stafford, or, perhaps, in the fact that we possessed in the late Mr. Richardson's "tunnel-handled dovetailed" modification of Holt's dilator, undoubtedly the best and safest instrument that has been devised for this form of treatment, and that the esteem in which we held the late Secretary of the Surgical Society made us slow to relinquish a plan to which he seemed so attached, and for which he claimed so much. Whether the influence of our colleague's opinion was the cause of our somewhat dilatory abandonment of the operation may be a question; but I believe no one cognisant of the general practice of surgeons in Dublin will deny that divulsion has fallen into comparative disuse, and I hope no one will deny that it has been supplanted by a better operation.

In my remarks I need make no lengthened reference to perinæal section, for, in spite of the somewhat heroic advocacy which Syme gave to it, it cannot be regarded as a rival to internal division, and no surgeon would now contemplate performing it, except in cases where urethral fistula existed, or where internal operation had more than once failed. Sir Henry Thompson has made one of the gravest objections to it as an operation of selection for the cure of stricture when he pointed to the inability of an operator to reach a series of strictures through a perinæal wound. To be sure, Syme claimed that when the "master stricture" had been divided the minor contractions soon disappeared, but this is not a belief in which most surgeons will follow him.

Given, then, the case with which we have specially to deal—a well-established organic stricture—the question is, how may it best be treated by surgical means so as to secure the greatest immunity

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from return with the least possible risk from the operation? If it be tolerably recent, and if time and circumstances permit, gradual dilatation may be tried, and will probably be successful, if only for a limited period. We meet, however, many cases of recent stricture where frequent catheterisation will not be borne, or, even if it be, where the circumstances of the patient demand speedy relief, and in these no hesitation need be felt in recommending the division of the contracted portion of the urethra without delay—nay, more, in spite of the advocacy which gradual dilatation, when carried to an extreme degree, has of late years received, I am so convinced of its tediousness and uncertainty, and of the speed with which the contraction re-establishes itself as compared with those cases treated by division, that I would not hesitate to divide any stricture, however recent, provided it were confirmed, and either unsuited to the gradual treatment or disposed to re-contract after it, and that the kidneys were free from disease—any albuminuric condition in special being a good reason for avoiding, if possible, cutting operations, and trusting to the more temporary relief by gradual dilatation. Let me not be understood as advocating the relinquishment of gradual dilatation in all cases, but as proposing to limit its use considerably.

The bulk of cases, however, for which we are consulted are such as to save us any hesitation on the point of their recent occurrence. They are generally old, well-established, and have been thoroughly "cathetered," and in these—putting aside, for the reasons indicated, the method of gradual dilatation, unless forced upon us by the patient's dread of an operation, or by some exceptional circumstance—we have to decide between "divulsion" and "division," and if we select the latter, between a division from behind forwards, after the method of Civiale, or from before backwards, after that of Maisonneuve. For myself I have relinquished the operation of bursting stricture in favour of that of dividing it, and have most confidence in the method of Maisonneuve.

In instances where an urinary fistula complicates the existence of the stricture, it way be well to inquire if the minor procedure of an internal division should be attempted before undertaking a

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perinæal section. Observation of such cases leads me to believe that if the fistulæ are old-standing, or the perinæum is much riddled by them, nothing short of an external operation is likely to succeed; but if, on the other hand, the sinus is recent, solitary, and the perinæal tissues are tolerably healthy, an internal urethrotomy should be tried as a primary measure. In most of the cases, however, with which we have to deal this troublesome complication is not present, and our choice of operation is consequently limited.

In the early years of practice I performed the operation of immediate dilatation frequently, and the best expression of my opinion concerning it is found in the fact that I have completely relinquished it. I used Richardson's dilator altogether eighteen times, and have often seen it employed in the practice of other surgeons. Although I cannot say that I have ever found any grave effects follow its employment in my own cases, sufficient reasons exist for abandoning it. On the whole, it has been but little more provocative of danger or disturbance than Maisonneuve's operation; there are, however, other arguments against its use. It requires the stricture to be sufficiently dilated to permit the passage of at least a No. 3 or 4 bougie—a matter in itself often involving considerable delay or inconvenience; and although so high an authority as Sir H. Thompson has lately, when arguing in favour of internal urethrotomy being performed after the manner of Civiale, from behind forwards, stated that he considers such a dilatation to be a matter offering no impediment, I must take leave to differ from him; for, even in the most skilful hands, the enlargement of a tight stricture to this dimension is often tedious and troublesome. If, moreover, a surgeon believes that division is, at least, as safe a process as its rupture, he will at once concede that the quickest method is the best, and that the delay necessary for divulsion is a strong objection to it as compared with internal urethrotomy, even if no other existed.

But setting aside this objection, and others which I have not gathered from my own practice—such as the inflammatory troubles some surgeons have found to follow divulsions, or the question whether the mucous membrane is stretched or torn—there is the

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much more radical matter, that a return to the strictured condition is a mere matter of time. It is true that this objection may obtain to any operation, and that all surgeons are agreed about the uncertainty as to re-contraction after even the best devised methods; yet there can be no doubt that return of stricture is as much the rule after divulsion as it is the exception after urethrotomy. This cannot be a matter for surprise; for if the operation produces, as is sometimes claimed for it, a dilatation without rupture, what more likely than a re-contraction of the tissue; and if, on the other hand, it bursts the stricture, as it does often, if not always, it is not difficult to understand that the copious lymph exudation following so rude a process must result in subsequent narrowing of the passage. I have found contraction to become re-established at very various and often very short periods. A few cases have been lost sight of before any considerable contraction had taken place, but in the majority it has shown itself in periods varying from a few weeks to a few years. In nearly every instance where a stricture has been burst it will be found, on careful measurement, that, in a space of time to be counted by days, the passage which, immediately following the operation, admitted, say, No. 12 or 14, will not easily take more than 10 or 12. Although this is not a return to an actually strictured condition, it is sufficiently indicative of what may be expected later on. In fine, so unfavourable has been my observation as to the permanency of relief afforded by the method of divulsion, that it has induced me to set aside a proceeding of such unsatisfactory result.

Of late years I have confined my treatment of those cases of stricture suited for internal operation to the method of division by a cutting instrument, and the performance of my twenty-fifth case of Maisonneuve's operation without any death or instance of extreme danger is my reason for making this communication, and for repeating the praises of the treatment which have been so often uttered. It is satisfactory to find that the great bulk of surgeons are agreed as to the advantages, in suitable cases, of internal urethrotomy over other methods of treatment, and that the leaders of the profession, both at home and abroad, wage their war over

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the minor matter of the particular form of operation to be selected. Thus, while many men of the highest eminence warmly advocate the operation done after the fashion of Maisonneuve, from before backwards, we find Sir H. Thompson, the person in England of the largest experience, advocating the method of Civiale, and cutting from the vesical side of the stricture. In the admirable lectures he delivered before the College of Surgeons last year, he entered at length into his reasons for choosing this operation; but it is difficult to follow him on this point in his arguments or his choice, for I hold that with Maisonneuve's instrument a stricture can be divided with at least as much certainty that it has been cut thoroughly as with any instrument devised for cutting from behind forwards; and I refuse to recognise the truth of this author's remark—that the system of Maisonneuve "is the product of a machine, the other is the handicraft of an artist." Sir Henry Thompson's aphorism, "If you cut at all, cut all," is a part of his teaching no one will deny the wisdom of; but I am satisfied it can be obeyed most thoroughly with the instrument I am in the habit of using. As Mr. Berkeley Hill has said, the great objection to most urethrotomes is the difficulty of using them so as to effect a thorough division of the fibrous tissue, on account of the want of means of fixing the stricture in the perinæum. The method of Maisonneuve is more free from this drawback than any other, as the firm hold which can be taken of the penis during its use, and the fixation of the stricture against the edge of the knife thus obtained, make an argument in favour of the operation which is not to be overlooked.

In addition to the foregoing reasons two others may be adduced in favour of Maisonneuve's instrument. One is that the pliant nature of the steel directing rod allows the contrivance, when the surgeon is pushing on the knife, to adapt itself somewhat to the position of the passage, and that while it has sufficient rigidity and resistance to enable him to feel what he is doing, it saves the tissues neighbouring on the corpus spongiosum from unnecessary division. The other is the well-whipped subject of the size—No. 5 or 6—to which a stricture must be dilated before Civiale's instru-

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ment can be used; and of this it is enough to say that, in spite of the high authority of the surgeon already quoted, it is, *cæteris paribus*, of advantage, and often of the greatest moment, to be able to operate at once on a tight stricture, as Maisonneuve's instrument permits. For instance, I lately did the operation, with complete success, on a gentleman suffering from acute retention, who had lacerated his urethra in his efforts to pass a small catheter, who was in a state of high fever, and who could not, before operation, bear to retain even the smallest instrument without rigors and other alarming symptoms. I can now look back over about twelve years on the cases operated by Maisonneuve's method, and, without entering into a wearying detail, I may say generally that the results have given me great satisfaction. I have never had a death, nor have I seen one except in a case under the care of a colleague, where the operation was done on a man in advanced phthisis, who had sustained a rupture of his urethra when straining to pass water through a tight stricture. This statement has all the more weight because the operation is one in favour with my colleagues, and very frequently performed in the Richmond Hospital.

I have not had cause for much anxiety concerning my cases except on a few occasions—one, the instance just mentioned, in which very troublesome hæmorrhage took place, and required the retention of a large catheter, to the temporary detriment of the patient; and the others, five in number, where some troublesome rigors and feverish symptoms showed themselves after operation. In two of these the subjects were addicted to drink; and in all a full-sized catheter had been introduced and retained in the urethra —a practice which, unless demanded by hæmorrhage, I have now abandoned. Such of my patients as have been kept in view remain in a favourable condition, and will, no doubt, continue to do so, if they follow the rule laid down for them as to the occasional passage of an instrument.

In none of the eight cases I have operated on in private has any return of the stricture as yet showed itself; they have been cut at periods dating back from six months to ten years, and were all

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tight, old-standing strictures, with a good deal of bladder trouble. In one instance the stricture had existed for thirty years; the bladder was greatly hypertrophied, and the gentleman had not passed a day for twenty years without one or more catheterisations. He can now eject his urine in a stream, which is a source of pleasure and pride to him, and enjoys a comfort to which he had been a stranger.

My hospital cases, which are dotted over the last twelve years, have, as a rule, not been of such old standing, and have not all been kept in sight; but, as the patients have always been instructed to return on the least suspicion of a re-contraction, I conclude that such as have not returned are free from stricture. Of those kept in view (about ten in number) only one has had a return. He is an intemperate man, of about forty years of age, who was cut four years ago, and in whom I then passed a No. 14 bougie. At times, when he is sober, he now passes a fair stream, and a No. 6 or 7 can easily be introduced. At periods of intemperance he has, as might be expected, considerable difficulty in voiding urine, due to the urethral congestion which is then present.

As regards the method of operation, reference need only be made to such matters as are moot points, and about which surgeons differ. The preparation of the patient is, of course, of importance. In addition to attending to the state of the urine, and, if possible, securing an acid or, at worst, a neutral reaction, the matter of most weight is to give due rest to the parts by keeping the person in bed for a few days. In men otherwise healthy, or if urgent necessity exists, there need be no hesitation in dispensing with this preliminary, and I have occasionally operated in my own study without any preparation except the previous clearance of the bowels.

An anæsthetic is usually administered; this is not necessary. In most cases the operation gives very little pain, not more than the introduction of a catheter for the first time into a tender urethra. The callous condition which the urethra has assumed is explained by the fact that it has in these patients been the seat of long-standing inflammation, and has lost its natural sensibility. Twice,

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in the cases of persons who were very nervous, and who pressed for the use of an anæsthetic, I consented, and found nitrous oxide sufficient and satisfactory. The director was introduced before the gas was administered, and the stricture readily cut before its effects had passed off.

The size of the knife to be selected is an important point, and should be determined by previous careful measurement of the calibre of the urethra. It is particularly necessary to avoid the fallacy of being guided by the size of the meatus, which, if small, should be divided, and a careful exploration of the passage made, so as to determine the number and size of any strictures that exist. Failing the determination of the normal size of the canal by bougies or sounds, it is best to form your opinion by the use of Otis's rule, and to be guided by the circumferential measurement of the penis. In practice I have found generally that if I erred it was in using too small a knife, and now I usually employ the largest blade ordinarily supplied with Maisonneuve's instrument.

The surgeon has next to decide whether he will incise the floor or roof of the urethra. Differing from Sir H. Thompson, and agreeing with Professor Otis on this point, I prefer to make my cut on the pubic wall, and for this purpose use that form of Maisonneuve's urethrotome which carries the blade along its concavity. There is but one reason to be adduced for the inferior incision—that the greater thickness of that portion of the corpus spongiosum lying below the urethra renders it less likely that the knife will go completely through it, and, entering neighbouring tissues, produce extravasation. Against this are to be set the facts relied on by Otis, that the superior wall is less vascular than the floor, and its incision therefore less likely to produce troublesome hæmorrhage; and, again, that as the tissues involved in the stricture are much thicker below the urethra than above it, an incision sufficiently extensive to divide it completely is more likely to cut into neighbouring parts than if made above. Besides, it seems that an incision made through the roof of the urethra into comparatively healthy tissue is less likely to be followed by contraction than when carried through the more unhealthy structures below, infiltrated as

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they are by lymph. The larger amount of erectile tissue situated beneath, with its freer condition, and the consequent ease with which its infiltration is effected, together with the greater liability to disease generally found to exist on the inferior as compared with the superior aspects of tubes and cavities, accounts for the frequency with which inflammatory trouble limits itself to the floor of the urethra. The stricture having been thoroughly divided by pushing the knife backwards through it, more than once if necessary, until all sense of resistance ceases, the instrument is withdrawn, and a full-sized catheter—generally 14 or 16—passed into the bladder.

Most surgeons tie in the instrument and leave it for twenty-four or forty-eight hours, after which it is withdrawn and passed daily at first, and then at longer intervals. From this practice I am compelled to differ; most of the troubles I have seen following the operation have resulted from leaving in the catheter, and I have relinquished this habit, and, having passed the instrument at the conclusion of the operation, I withdraw it. Since adopting this plan I have found much less constitutional and local trouble follow the operation.

In favour of the retention of a catheter in all cases after the operation it is urged that it lessens the likelihood of extravasation. This is more than doubtful, and I think that those who have seen the way in which the outside of a catheter acts as a conductor of urine, and in which that fluid will escape along the side of even a full-sized instrument, will agree that it is as likely to help extravasation as to prevent it. Of course, if hæmorrhage occurs a large catheter should be tied in. Commencing twenty-four hours subsequent to operation, the catheter should be passed daily for about a week, after which the patient may be taught to introduce it himself. I generally direct him to use it every second day for a month, and afterwards to pass a No. 12 or 14 about once a week for the rest of his life.

This last precaution is of the greatest moment; it generally prevents subsequent contraction, or will, at all events, give the earliest warning of it. All surgeons of experience agree that there

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is no certain immunity from the return of any organic stricture, and this cannot be too strongly taught to our patients, and used to impress on them the necessity for the periodic use of a catheter or bougie. Let a man use one every Saturday night and he will probably keep the canal sufficiently dilated for easy micturition. The most suitable instrument is a large olivary bougie—14 or 16 if possible.[a] A bougie is better than a catheter, because it lasts longer and is more easily kept clean.

The amount of preference which has of late years been given to internal urethrotomy, and the way it is beating other operations out of the field, are reasons which have induced me to make this communication, and to endeavour, by a pebble dropped by the road, to define an advance in treatment. It is quite true that many surgeons in Dublin practise this operation in all possible cases, but very little has been written by them about it; and, as we mark the epochs of surgery more by the pen than the knife, it is, perhaps, not out of place that I should record what I believe to be the opinion of most of us on the subject.

It is, then, not only to report my own experience in the treatment of stricture that I have brought the subject under the notice of the Academy, and recorded my opinions on certain matters of detail about which a difference exists among surgeons. The points about which others may disagree with me, are:—

1. The superiority of the operation of internal urethrotomy over any form of dilatation.

2. The advantage of the method of Maisonneuve over that of Civiale and his followers; and

3. Certain details connected with the method I have advocated, such as—(*a*) completeness of division; (*b*) the disuse of anæsthetics; (*c*) the position of the incision; and (*d*) the non-retention of the catheter in the urethra subsequent to operation—the latter, in particular, being a matter on which I place much weight, to which I attribute the freedom of my later cases from inflammatory and other troubles, and about which I am most at variance with the general practice of other surgeons.

[a] All sizes of catheters referred to are on the English scale.

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OBSTETRICAL SECTION.

FOURTEEN CASES OF OVARIOTOMY.

By ARTHUR V. MACAN, M.B.;
Master of the Rotunda Hospital.

[Read in the Obstetrical Section, January 9, 1885.]

In the paper I have the honour of reading before the Obstetric Section of the Academy to-night, I purpose, first of all, to give, briefly, the details of fourteen cases where I myself have performed ovariotomy, and then to draw attention to some points in practice which they seem specially to illustrate, and to which, in my opinion, the attention of the general profession in this country has not as yet been sufficiently directed.

The first two cases of the series were operated on in the City of Dublin Hospital—the other twelve in the Rotunda Hospital. I have to thank Drs. Fitzgibbon and Wheeler for their kindness in assisting me in the two former, and Drs. Bennett, Corley, Atthill, and Smyly for their help and assistance at the cases operated on in the Rotunda Hospital.

I will now give a brief *resumé* of these fourteen cases:—

Case I.—M. A. O'K., aged twenty-five, single, operated on in the City of Dublin Hospital on October 15, 1880, being sent to me by Sir William Miller, of Derry.

Operation.—A multilocular cyst, with considerable parietal adhesions. No fluid flowed through the trocar; so the cyst wall was incised and the hand inserted, and, after considerable difficulty, the cyst broken down and the tumour removed. Pedicle long and slender; was transfixed and tied in two portions.

This girl made a most perfect recovery.

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CASE II.—Mrs. S. L., aged forty; no children. Small cyst, about size of uterus at five and a half months' gestation, filling the right side of pelvis, and pressing the uterus over to the left. No sulcus could be felt between the fundus and the tumour, nor could the latter be raised in the slightest degree from the brim of the pelvis. The diagnosis was, therefore, made of a very short pedicle, and probably an intra-ligamentous growth. At the operation (which was performed on June 2nd, 1882) both these suppositions proved to be correct; and there being no pedicle, I passed a double ligature as low down on the base of the tumour as possible, and tied the pedicle thus formed in two parts, and cut the tumour off at some distance above it. The pedicle, in fact, was composed of the walls of the cyst itself, and on this account I inserted a glass drainage-tube before closing the abdominal walls.

The highest temperature this woman had for three weeks after the operation was 100·6°. The drainage-tube I allowed to remain in too long, and a good deal of suppuration took place, pus—and by no means sweet-smelling pus—flowing freely from the wound in the abdomen. But, notwithstanding this, the temperature remained practically normal.

On the third day I washed out the peritoneum with carbolic acid solution, after which she had well-marked carbolic urine.

This woman was admitted afterwards into the Rotunda Hospital, the fistulous tract being still unclosed, and died six months after the operation of chronic peritonitis, complicated with cancer of the liver.

CASE III.—Mrs. E. S., aged twenty-three; sent to me from Preston by Dr. Robinson, an old pupil of the City of Dublin Hospital.

The tumour, a large unilocular cyst, had been noticed by the doctor who had attended her in her last confinement, two years previously, and was at first taken for a second child. The operation (which was performed on December 18th, 1882) was entirely uncomplicated, the cyst containing twenty-two pints of a dark fluid. Her temperature, nevertheless, rose much more than the average height during convalescence.

CASE IV.—E. F., aged fifty-one, single. This woman was a patient in the Rotunda Hospital when I took charge of it. Dr. Atthill had not operated on her, either from there being some

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uncertainty in the diagnosis, or from not looking on her case as a favourable one. She was greatly reduced before the operation, and though there was no hæmorrhage, I feared she would die on the table. The tumour was an intra-ligamentous cyst which had developed between the folds of the left broad ligament, and had no pedicle. I passed the ligature as low down as possible, and removed the tumour above the ligature. This woman was operated on on February 2nd, 1883. She never recovered from the shock of the operation, and died at 10 30 p.m. on February 4th, her temperature never having risen as high as 100°.

Case V.—M. B., aged twenty, single; sent up by Dr. Curtis, of Cork. The tumour in this case had grown very rapidly since it was first noticed, which was six months before she was admitted into the Rotunda Hospital. She was operated on on March 24th, 1881, Dr. Curtis being present. I should have mentioned that there was a considerable amount of albumen in the urine, and the abdominal walls were greatly thickened by œdema. There was great difficulty experienced in withdrawing the tumour from the abdomen, as part of it was very solid; when an attempt was made to break it down by passing the hand into the interior of the cyst this was found impossible, large spicula of bone projecting in every direction. There were extensive omental adhesions, some containing arteries nearly the size of the radial. After the removal of the tumour the other ovary was found also to be degenerated, being about the size of a hen's egg. It was quite sessile, and contained a lot of cheesy matter mixed with hair. It was ligatured with difficulty, and then removed. The tumours, when further examined, proved both to be dermoid cysts. The patient seemed at first to be doing very well, but at 7 30 p.m. on the day following the operation, her pulse was 160 and hardly to be counted, her temperature being 102·5°. As she seemed to be dying of septicæmia, I determined to re-open the abdomen and insert a drainage-tube. Dr. Bennett kindly came over and assisted me in doing this. Very little fluid was drawn out of the abdomen, but at 12 p.m., less than four hours after the operation, the pulse had fallen to 132, and the temperature to 101·3°; the next morning the pulse was 122, and temperature 100·6°. This girl had afterwards some ups and downs from the formation of an abscess in the abdominal walls, which caused a considerable rise of temperature as late as the sixteenth day, but finally made a perfect recovery.

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Case VI.—Mrs. B. C., aged twenty-nine; admitted into the Rotunda Hospital on May 11th, 1883. This woman had first noticed a swelling about two months after the birth of her fifth child, just a year before her admission into hospital. She had suffered from frequent attacks of pain in the abdomen, accompanied by vomiting and rise of temperature. She was operated on on May 25; the cyst was a multilocular one; there were most extensive intestinal adhesions, which were so troublesome that the operation lasted for two and a half hours. On account of the extensive adhesions a glass drainage-tube was introduced. The intestines were in this case exposed for a long time to the carbolic spray, and the woman had smoky urine and a good deal of vomiting for some days, but her temperature never rose above 101·4°, her pulse at the time being only 88.

Case VII.—M. D., aged nineteen, single. Admitted to the Rotunda Hospital on June 12th, 1883. Operated on on June 29th. Unilocular parovarian cyst; no complications. This girl made a splendid recovery, her pulse never rising above 104. About three weeks after the operation an abscess formed in the abdominal walls. She returned to the hospital four months afterwards, complaining of pain in the abdominal wall. On examination a small fistula was observed about the middle of the wound, and in it was found a silk ligature, lying quite loose. This was removed without difficulty, and the fistula soon closed.

Case VIII.—M. J. J., aged twenty-eight, single; sent over from Holyhead by Dr. Maguire. Admitted into the Rotunda Hospital on January 2nd, 1884. Operated on on January 10th. Tumour, a large multilocular cyst with parietal adhesions; part of the tumour projected down into Douglas' space, but was fortunately not adherent there. The fluid was very viscid, and a considerable quantity escaped beside the trocar into the abdominal cavity. The maximum temperature was on the third day, when it reached 100·6°, and patient never had the slightest bad symptom.

Case IX.—M. M., aged twenty-four, single; admitted on January 11th, 1884, from Galmoy, county Kilkenny; sent by Dr. Duckworth, Donaghmore. Operated on on January 25th, 1884. Tumour, a unilocular cyst. Pedicle was long, but in tying it the silk broke, and a good deal of blood escaped from the puncture in the broad

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ligament. A second ligature was passed as nearly as possible at the same place, and tied firmly, when all bleeding apparently ceased. This patient went on well for twenty-four hours, after which time the pulse and temperature rose rapidly, so that at 4 p.m. next day the former was 156, and the latter 101·6.° I therefore opened the abdominal incision thirty-four hours after the operation, and introduced a drainage-tube, through which I removed more than an ounce of seemingly quite pure blood. As the operation had been quite uncomplicated, it was obvious that the hæmorrhage must have come from the puncture in the pedicle, which bled after the abdominal wound was closed. The next morning, at 8 a.m., her pulse was 120, and temperature 99·4°, and from this the patient went on very well till the eleventh day, when she had a sudden rise of temperature to 102·7°, and pulse 120, with great pain along the lower border of the right ribs and great dyspnœa, followed by bloody sputa. This, I thought, was most probably due to infarction—a clot in the pedicle having got loose and been stopped in the lungs. There was a very similar rise of temperature, with dyspnœa, on the nineteenth day, after which time patient gradually recovered, and was sent home quite well on March 9th, 1884.

Case X.—Mrs. S. S., aged thirty-two; admitted January 23, 1884; sent by Dr. Lyster, of Kilkenny. This woman was two and a-half years married without having any children. Operation on February 19th. Unilocular cyst; no complication. Recovery perfect. Maximum temperature on evening of the operation, when it rose to 100·6°, and pulse 116.

Case XI.—Mrs. K., aged fifty. This woman was a former patient of mine in the City of Dublin Hospital, having been sent by Dr. Hugh Byrne, of Lower Baggot-street. She had been tapped three times, and for many months after the third tapping the abdomen did not increase in size.

She was operated on on March 25th, 1884, Drs. Bennett and Corley assisting. This was by far the most difficult ovariotomy I ever did. It was impossible to distinguish accurately which was peritoneum and which was the cyst wall. A considerable portion of peritoneum was, therefore, separated from the abdominal walls at the lower angle of the incision before the mistake was discovered. The cyst was almost universally adherent, and failing to find the line of demarcation between the peritoneum and cyst wall, I passed

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my hand into the cyst, and tried to turn it inside out. While doing this my fingers passed through the cyst walls and got into the cavity of the abdomen. I was still, however, unable to distinguish the line of separation, and finally the cyst was removed piecemeal, numerous shreds remaining adherent to the abdominal wall. The peritoneum that had been separated from the abdominal wall at the lower angle of the wound was then carefully sewn back in its place, a drainage-tube inserted, and the abdominal wound closed. The operation lasted two hours and twenty-five minutes.

This woman had a quick pulse and a high temperature for some days, but finally made a thorough recovery, and I saw her some time ago, seemingly in perfect health.

Case XII.—K. K., aged twenty, single; admitted on July 8th, 1884; sent up by Dr. Cordner, of Aughnacloy. Operation performed on August 1st. Large multilocular cyst; some slight parietal adhesions. This woman made an uninterrupted recovery, the maximum temperature being 101° on the day after the operation.

Case XIII.—S. F., aged twenty-nine, single. Sent up by Dr. Palmer, of Crossmaglen, Co. Armagh.

On admission a hard tumour, the size of an orange, could be felt, rising out of the abdomen, just above the right Poupart's ligament. On making a vaginal examination this was found to be the fundus uteri, which was pushed upwards and to the right by a large elastic tumour filling up the left side of the pelvis. The girl was therefore put under ether, and on pressure being made on the tumour it suddenly rose with a jerk into the abdominal cavity, and the uterus fell down into its place. The diagnosis was therefore made of a small ovarian tumour, and, from the ease with which it could be moved about, it obviously had a very long pedicle. After the examination the girl's temperature rose, and as, from the length of the pedicle, and the globular shape, and the very free mobility of the tumour, I suspected twisting of the pedicle, I determined to operate as soon as possible. The operation was performed on August 19th, 1884. The tumour was about the size of a child's head and was of a dark blue colour, due to congestion caused by three or four twists on the pedicle. On the first day after the operation the temperature rose to 102·4°, but the next day it was down to 99°, and from that on was practically normal. This was the smallest tumour I ever removed, and it is hard to account for

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the length of the pedicle in any mechanical way, as the tumour was evidently fixed in the pelvis.

Case XIV.—Mrs. J. T., aged forty-eight; admitted September 20th, 1884, being sent by Dr. Atock. Had been tapped about ten days previously. Has had six children. On October 6th she was put under ether and examined by the rectum, when the diagnosis of a left ovarian tumour, with a short thick pedicle, was made. The operation was performed on October 10th. As soon as the peritoneum was opened a large quantity of a thick reddish fluid escaped from the wound—evidently from some rupture of the sac. There were some very old and firm adhesions to the intestines, and the whole descending colon was attached to the cyst wall by more recent adhesions, the whole surface of which bled freely when separated. The pedicle was thick and short, and was tied in three pieces. The abdominal cavity was then sponged out, and while the abdominal wound was being stitched the woman ceased breathing, my attention being drawn to her condition by the very venous colour of the blood which was escaping from the edges of the abdominal wound. She soon came round under artificial respiration, which did not interfere with the closing of the wound. At this time her pulse was 120, temperature 96·4°; but in the evening her pulse had fallen to 94, and her temperature had risen to 100°. Next morning the temperature was 99°, and in the evening 99·2°, which height it never again exceeded. This was, perhaps, the most perfect recovery I had ever had, after what, at the commencement of my career, I would have looked on as a fatal accident—viz., the free escape of the cyst fluid into the abdomen.

The strictest antiseptic precautions have been invariably practised at these operations, but lately I have not directed the spray on to the abdominal wound—indeed it is now chiefly used with the view of disinfecting the lookers-on. Mr. Lawson Tait has, it is true, obtained most excellent results without using any antiseptics, by operating, in fact, aseptically instead of antiseptically. But it is not everyone who can command such favourable surroundings as Mr. Tait; and for the great mass of the profession the use of antiseptics must, I fear, still remain a painful and troublesome necessity. No patient died of septicæmia, the only case I lost (No. 4) having succumbed to shock sixty hours after the operation.

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This gives a mortality of a fraction over seven per cent., which is, I think, an unusually favourable one to commence with; and, judging from the statistics of other operators, I am justified in hoping that, with increased experience, even this will be very considerably reduced.

I would here venture to suggest that, after ovariotomy, there are probably, just as after labour, two forms of septic poisoning—viz., hetero-genetic and auto-genetic, or, as I would prefer to call them, primary and secondary infection. The former is due to direct inoculation of the peritoneum with septic poisoning during the operation, and generally proves rapidly fatal; the second is due to the introduction of bacteria into the peritoneum. For these latter, however, to produce septicæmia it is necessary that they should find within the abdomen a medium suitable for their development. This is usually furnished either by cyst fluid which has escaped into the peritoneum, or by blood from imperfectly-staunched adhesions, or, which is perhaps the most favourable of all, a mixture of blood and cyst fluid. Hence, the two great principles in operating are—first, to exclude all septic poison; and, secondly, to leave neither blood nor cyst fluid behind in the abdomen to act as a nutritive fluid for bacteria. In a paper by the late Dr. Marion Sims, in the *American Journal of Obstetrics* for April, 1880, in which he gives an account of his impressions after seeing Keith operate, he says:—"The lesson that I gathered from witnessing Keith's operation is—never to close the external wound till we have secured every bleeding vessel, every oozing point, and made sure that the peritoneum is perfectly clean and dry." We know now that the peritoneum is capable of absorbing large quantities of blood, as in cases of intra-peritoneal hæmatocele, and also of cyst fluid, as is shown by numerous cases of absorption and cure following rupture of an ovarian cyst. But if we leave no blood or other fluid in the peritoneal cavity, the chances of secondary infection are enormously reduced; in fact, it cannot take place except there is a secretion of fluid from the peritoneum, which seldom occurs. Now this whole question is very closely connected with the use of the drainage-tube. In three of the above cases (Nos. 2, 6, and 11)

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I inserted a drainage-tube at the time of the operation; in nearly every case, indeed I now think in every case, unnecessarily. For if the operation be performed antiseptically, the peritoneum is capable of absorbing any amount of fluid, or rags and tags of adhesions. In the two cases, on the contrary, where the drainage-tube was inserted subsequently, I cannot but think that the patients would otherwise have died; for in both cases the pulse had mounted to 160, and the patients presented all the appearances of rapid septicæmia. In both the symptoms had entirely changed within twelve hours after introduction of the drainage-tube. In the former of these cases (No. 5) very little fluid escaped through the tube out of the abdominal cavity, and I am at a loss what to attribute the favourable effect of the drainage to, except it be in diminishing the intra-abdominal tension, and thus lessening absorption. In the second case, however, I removed for several days considerable quantities of pure blood. This was evidently acting as a nidus for the development of bacteria, and when it was removed their activity ceased. This view will, I think, explain how a drainage-tube may in some cases be of use, but in other cases quite fail to save the patient. This secondary or auto-infection may, I think, be produced by the action of ordinary decomposition bacteria on easily decomposing material, especially blood. Hence it follows that we should, as far as possible, prevent the entrance of air into the abdominal cavity as supplying one of the two links in the chain necessary to produce auto-infection. We should, therefore, before closing the abdominal wound, make firm pressure on the abdominal walls, in order to expel any air that has unavoidably entered the peritoneum. Air in a dry peritoneum is readily absorbed without producing any symptoms, and blood or cyst fluid in the peritoneum, if not decomposing, is also perfectly harmless, but, combined, they produce secondary or auto-infection.

Following the example of many other operators I have discarded the waterproof sheet. The opening of the peritoneum is simplified by cutting boldly down to the sub-peritoneal fatty layer without using a director, raising the peritoneum between two pressure

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forceps, and then incising it. There is still considerable controversy as to the length of the abdominal incision. The old idea, however, that the length of the abdominal incision has a direct influence on the mortality is quite given up. Schrœder makes the incision in every case from the umbilicus to within as near the pubes as he can go without wounding the bladder, the position of which is known by holding up the abdominal walls to the light. This he does in order to have plenty of room, and to see exactly what he is doing. Mr. Lawson Tait, on the other hand, makes the incision as small as possible, on the grounds that a large incision is very likely to be followed by ventral hernia. This result, Schrœder says, he has never seen; and, as far as I can judge, the advantages of a large incision over a small one are very great. I would commence, in every case where there was a multilocular cyst, with an incision large enough to admit the whole hand.

A very serious question arises where the peritoneum and cyst wall are firmly adherent throughout the whole line of the incision. Here we may sometimes find the line of separation by enlarging the incision upwards. If this fail, we should boldly cut through the supposed peritoneum. For if it turn out to be cyst wall no damage is done; but if, on the other hand, we mistake the peritoneum for the cyst wall, and, under this idea, separate it from the abdominal walls, very serious consequences may follow. One great help is to remember that the cyst wall is devoid of all fat, whereas the sub-peritoneal connective tissue is full of it. If we should be unfortunate enough to make such a mistake, we should sew the peritoneum carefully back in its place, as was done in Case XI.

The usual method of emptying the cyst is by plunging a large trocar into it. Schrœder, however, and many other operators incise it freely with a scalpel. Even if cyst fluid enter the peritoneum it will do no harm; though if the assistant keeps the abdominal wall pressed against the tumour this even is impossible. In any case, should nothing flow through the trocar it should be at once removed, the cyst wall incised freely, and the contents of the tumour turned out with the hand. In Case XI., while thus emptying a tumour, I pushed my hand through the cyst wall, and,

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before I knew it, had a firm grasp of the intestines. If I had not recognised them at once, the consequences would probably have been fatal.

Perhaps the most difficult point in a case of ovariotomy is the separation of adhesions, and stopping the hæmorrhage from the separated surfaces. Slight parietal adhesions may be separated before the tumour is lessened; but the golden rule is to bring all intestinal and omental adhesions into the abdominal wound while separating them, and, with the aid of our sight, at once tie or otherwise secure every considerable bleeding point. If this be not done at the time, these bleeding points are impossible to find afterwards, and are thus a frequent cause of auto-infection. Intestinal adhesions require the very gentlest handling, as they are often very firm, and if roughly pulled on, the intestine will give way. We must therefore separate them, when firm, either by knife or scissors. A wound in the intestine is, however, by no means necessarily fatal if carefully sewn up, and we should always be provided with special needles and silk for the purpose. Oozing from large surfaces is best controlled by sponge pressure, or by a tight abdominal bandage. If in the abdominal walls, we can readily stop it by sutures surrounding the bleeding surface. Adhesions in the posterior *cul-de-sac* are peculiarly dangerous, because we cannot see what we are doing. Here sponge pressure is often of great service in stopping hæmorrhage.

The pedicle, if broad, should be tied in parts, as, if tied *en masse*, the peritoneal edges are very liable to slip out, which will cause secondary hæmorrhage. If there be no pedicle, as where the tumour is developed between the folds of the broad ligament, the capsule must be incised and the tumour enucleated. Olshausen, of Halle, has lately introduced the use of the elastic ligature for tying the pedicle. Up to April, 1884, he has used it in about eighty cases, and says the more he uses it the more he likes it. It certainly has the advantage that with it secondary hæmorrhage from the stump is almost impossible. The elastic ligature is returned with the stump into the abdomen, and causes no more irritation than the silk ones.

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In one case, No. 13, there was commencing gangrene of the cyst from torsion of the pedicle; and, fearing this complication, I operated, though the patient had a temperature considerably over 100°.

As to the rest of the operations little remains to be said. The toilet of the peritoneum is requisite exactly in the inverse proportion to our antiseptic precautions, and very nearly the same may be said of drainage.

In no case where there have been extensive adhesions, and hence, necessarily, oozing from large surfaces, should we omit to put some elastic dressing, such as carbolic tow or a large sponge, over the first dressing, and cover the whole with a firm bandage.

The after-treatment should be purely expectant. My rule in ordinary cases is to give nothing but small pieces of ice during the first forty-eight hours. If there be collapse, we should administer ether hypodermically rather than run the risk of vomiting by giving stimulants by the mouth. Pain and colic are best relieved by morphia hypodermically. The stitches should be removed on the tenth day, up to which time, if the case be going on well, the dressing need not be disturbed.

In conclusion, let me say that success in ovariotomy is not the exclusive possession of any class, whether they be surgeons or specialists, but will follow him most frequently who to quickness, gentleness, and strict discipline during the operation, adds a thorough knowledge of the various complications that may arise and the methods by which they have been met and overcome by other operators, and has, above all, a due appreciation of the value of antiseptics in abdominal surgery.

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CASE OF SUPPURATING OVARIAN CYST COMPLICATED BY THE EXISTENCE OF A DEEPLY-SEATED PELVIC ABSCESS.

By LOMBE ATTHILL, M.D.;
Ex-Master of the Rotunda Hospital.

[Read in the Obstetrical Section, Jan. 9, 1885.]

On the 26th November, 1884, a lady came to town to undergo the operation of ovariotomy. She was sent to me by Dr. Moran, of Gort, who had previously diagnosticated the existence of an ovarian tumour.

On examination I was satisfied that the tumour consisted mainly of one large cyst, but there was also a solid portion which lay in the left inguinal region. The case seemed a favourable one for operation, and as the patient was very anxious to have the tumour removed I decided to operate at once; but a sharp attack of bronchitis, the result of cold caught during her journey, supervening, the operation was necessarily postponed. On the bronchitis subsiding, she was attacked by peritonitis, which required the application of leeches and the free exhibition of opium. The fever was high and there was much pain.

As these symptoms subsided I was much disappointed to find that her condition did not improve as I had hoped; and I now observed a new phase in her illness: hitherto the temperature had ranged from 101° to 102°, now at night it rose to 103·5° to fall to 98° in the morning, the pulse ranging from 140 to 120 in the minute; the tongue, too, became dry and glazed, and I came to the conclusion that the contents of the tumour must be undergoing a change.

Dec. 21st.—Her condition now was most unsatisfactory, and indeed alarming. Her illness had lasted four weeks; she was very feeble and greatly emaciated, the pulse small and rapid, the temperature, as already stated, varying 5° between night and morning. The urine was very scanty and high-coloured, only eight ounces

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being secreted in twenty-four hours. There was great thirst, and any attempt to take food, other than milk and soda-water or beef-tea, was followed by vomiting—in fact, nothing could be more unfavourable than her state for the performance of a capital operation. It was evident, however, that in the condition the patient was in she could not long survive, and that the only hope of saving her life rested on the speedy removal of the tumour. The patient willingly agreed to undergo the risk, and was operated on on the morning of the 23rd of December. Ether was administered by Dr. Charles Ball, and I was assisted by Professor Stokes. The atmosphere in the room was thoroughly impregnated with carbolic spray, but it was not allowed to play on the abdomen; all other antiseptic precautions were strictly observed.

The first part of the operation presented no difficulty. The large cyst was rapidly exposed and emptied. There was nothing exceptional in the fluid contained in the large cyst, but on proceeding to break down the small cysts, which composed the more solid portion of the tumour, they were found to contain pus. The diagnosis that the cyst was a suppurating one was therefore verified.

But although the cyst was thoroughly emptied of its contents, it could not be drawn forward, and I now found that its base was intimately adherent to the pelvic walls, both laterally and posteriorly. The task of breaking down the adhesions was tedious and difficult. Those to the right were first separated, then those posteriorly, but while breaking down those to the left, at a point corresponding to the solid portion of the tumour, in which were situated the suppurating cysts, I was disagreeably surprised at finding a quantity of by no means inodorous pus to well up. At the moment I assumed that I had torn the wall of the cyst, and that the pus had been contained within it, and it was not till some time after the operation was over that I discovered, on a careful examination of the tumour, that it had been removed in a perfect condition. The pus, therefore, must have come from a deeply-seated pelvic abscess.

The base of the tumour was at length freed from its attachments, drawn forward, and separated. The pelvis was now full of blood and pus, which was carefully removed, and the cavity washed out with a five per cent. solution of carbolic acid.

Blood, however, continued to ooze freely from the torn surfaces, and pressure by means of sponges had to be made on them for a short time before it was arrested. The wound was then closed, but I deemed it wise to insert a drainage-tube, my reasons for

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doing so being that the oozing of blood had not absolutely ceased, and also because a large quantity of pus having escaped into the pelvis the danger of septic poisoning was greatly increased. The result fully justified this step.

The patient was in a state of extreme collapse at the termination of so tedious an operation, but she soon rallied, and in two hours complete reaction was established.

At 3 p.m. the temperature was 102°, at 10 p.m. it had fallen to 99·6°; pulse, 130. There was no vomiting, and the urine, which had been very scanty for some days previously, was now freely secreted. Little pain was experienced. A quantity of serous fluid escaped from the drainage-tube, and at 10 p.m. I removed through it, by means of a syringe, nearly twelve ounces of bloody fluid, which let fall rapidly a copious deposit, consisting mainly of blood and pus corpuscles. The fluid thus removed had a very heavy sickening smell. The patient passed a very quiet but sleepless night.

During the next day all went on well. The temperature did not rise above 100°; the pulse, however, was feeble, and 130 in the minute. About three ounces of fluid similar to that removed the previous day was sucked out through the drainage-tube.

On the morning of her fourth day (the 26th Dec.) her state was so satisfactory, and such a small quantity of fluid could be withdrawn from the tube, that I removed it, laying a fold of carbolised gauze over the place it had occupied in the abdominal wall, but on visiting her at 10 p.m. that night I found matters altered for the worse. The temperature had risen to 102° and the pulse to 130. I at once decided to reopen the communication with the cavity of the abdomen, but instead of again inserting the drainage-tube I passed into the pelvis a No. 12 gum elastic catheter, attached to an exhausted rubber bag, and sucked out six ounces of fluid of similar character to that previously removed, but with a much more fæculent odour.

27th.—She had a very restless night, but the temperature fell to 100°; the tongue had again become dry and glazed, there was no pain or tympany, and flatus was passed per anum. I again introduced the catheter, and removed a couple of ounces of fœtid fluid, so fœtid that I at once decided on washing out the cavity with a disinfecting lotion, and with this view injected several ounces of carbolic solution of the strength of about 5 grains to the ounce, and this was repeated twice daily till the 2nd of January, when I substituted for it a solution of the permanganate of potash.

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For several days subsequently her condition remained unchanged. On the 28th the bowels were moved by an enema, and a large quantity of flatus passed; there was not the slightest symptom of peritonitis, but the pulse continued small and rapid, 120 to 130 in the minute, and the temperature ranged from 100° in the morning to 102° at night. The tongue was dry and glazed, and there was much thirst.

On the 29th I was obliged to place her on a water-bed, being apprehensive of bed-sores forming. The fluid removed from the abdomen became less in quantity, but more and more like pus, and at the same time more fœtid, and about the 31st (that is, the 9th day) could not be distinguished from the pus met with in abscesses formed in close proximity to the rectum. I have seldom indeed met with more markedly fœtid pus.

During all this time, and up to the 14th day (that is the 6th January) she had taken nothing but beef-tea or soda-water and milk, one ounce of one or other being given every alternate hour; but on the 6th her tongue had become so moist and clean that I allowed her a little bread and butter with a cup of tea. This she enjoyed very much, and I considered all danger over (it being the 15th day since the operation). The bowels had moved without discomfort every second day during the previous week, an enema of tepid water being given to assist their action. Judge, then, of my disappointment on being hastily summoned at noon on that day, and to find that fæcal matter was being discharged through the opening in the abdominal wall. The nurse was in the act of administering the enema, and had only injected about eight ounces of water when the patient called out that the fluid was coming through the opening in the abdomen, and so it was, and it was evident that the wall of the intestine had softened and given way. That the opening was valvular was proved by the fact that though fæcal matter passed into the pelvic cavity no pus passed the reverse way—namely, from the pelvic cavity into the rectum.

From the date of this unfortunate occurrence—namely, 8th January—the patient's recovery was steady, but very slow. Of course the exhibition of enemata had to be discontinued, and, as the patient absolutely refused to take pills, it was a matter of difficulty to get the bowels to act satisfactorily. When the motions were solid no fæcal matter passed through the fistulous opening in the abdominal wall, but if they were liquid more or less always did so. As the opening in the abdominal wall showed a great

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tendency to contract, I inserted a short piece of vulcanite drainage-tube, which the patient was taught to remove and cleanse daily.

After the lapse of a few weeks the patient was well enough to travel to the West of Ireland. I saw her again on the 10th of May. During the interval she had greatly improved; she had gained flesh, looked healthy, and was able to take a little outdoor exercise daily. She still wore the drainage-tube, and occasionally a small quantity of fæcal matter was voided through it, but for several days together this was absent, and the bowels acted with tolerable regularity. It was therefore evident that the opening in the wall of the intestine was closing.

Recently I have received a letter from my patient, stating that there had been no fæcal discharge for a month, and this even though she had suffered from diarrhœa for several days. Menstruation had recurred normally, the drainage-tube had been withdrawn, and the opening in the abdominal wall all but closed—indeed except a slight oozing of mucus during the menstrual period there had not been any discharge whatever from it, while her general health had become good. It is therefore evident that the opening in the wall of the intestine had closed.

The operation in this case was undertaken as a last resource, under the most unfavourable circumstances. The diagnosis of suppurating cyst was verified, but the existence of a deeply-seated pelvic abscess was not suspected—indeed I do not see how it could have been diagnosed, as the size of the tumour rendered an exploration of the pelvic cavity impossible. The occurrence of the recto-pelvic fistula was probably due to the fact that the wall of the intestine was in contact with the abscess and had become softened as the result of the inflammation which had been set up in its vicinity, and possibly in which it had been implicated.

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HEMIPLEGIA AFTER DELIVERY.

By WILLIAM C. NEVILLE, M.A., M.D., M.Ch., & M.A.O., Univ. Dubl.; M.K.Q.C.P.I.

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[Read in the Obstetrical Section, 1885.]

The following case presents so many points of interest that I have thought it worthy of recording before this Section:—

Case.—*Left hemiplegia on morning after delivery in a 7-para.; aortic valvular disease and fatty degeneration of the heart; death in a fortnight.*—E. C., aged thirty, married ten years, was confined of her seventh child on the morning of the 7th December, 1879. Labour was normal. Previous labours had also been normal. A few days after the birth of last child, eighteen months ago, she is stated to have suffered from some form of eclamptic attack, attended with insensibility; the lochia then stopping permanently upon the second day. She had, however, recovered perfectly, and nursed the child until it died at four months. For the past year she has endured much hardship, her husband having been out of employment, "on the tramp."

During the evening after delivery she was restless, talking somewhat incoherently and sleeping scarcely at all during the night. On the following morning (8th instant) she felt much better, talked quite rationally, and complained only of some slight headache. She sat up in bed, and was combing her hair, when she suddenly felt giddy, fell back, and "was convulsed." The convulsions lasted only a short time, and were followed by partial insensibility and heavy breathing.

I saw her about two hours afterwards; she was then completely hemiplegic on the left side, the limbs of which were rigid, the left leg being extended and everted. The right limbs were subject to frequent choreiform movements, but could be moved at will. The head was kept sharply bent to the right side, towards which also both eyes were directed. The right pupil responded normally to light, but the left remained constantly dilated. There was

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well-marked paralysis of the left side of the face. Though some amount of stupor existed, the patient could easily be roused to understand questions, to which, however, she was quite unable to give any articulate reply. The power of speech was not completely lost, as she was still able, with some degree of indistinctness, to utter certain words, and made obvious efforts, when roused to do so, to explain her symptoms. Her pulse was weak and irregular (80), and her temperature normal. Respiration was slow and laboured. No lochial discharge, and no abdominal tenderness, the uterus being well contracted, and milk having appeared in both breasts.

In the evening of the same day the affection of her speech, which had become very marked, had almost entirely disappeared. The movements of the right limbs were also much less frequent. The bladder had been naturally emptied. A little urine, drawn off with a catheter, proved to be quite normal. Pulse 84. Temp. 98·5°.

9th December.—Complains of severe headache on the left side of the head. Milk more abundant. No lochia. General condition unaltered.

10th.—Headache less. Bowels having been only once moved slightly since labour, she was ordered a soap and water enema, which acted well. The lochia being still absent, the vagina was syringed with warm Condy's solution, and turpentine stupes applied over the abdomen. Examination of the urine showed that it was acid in reaction, concentrated (sp. g. 1030), depositing a thick sediment of light-coloured urates. No albumen or casts.

The area of cardiac dulness is considerably increased, and the apex beat, badly marked and widely diffused, can be best felt on a line with, and about two inches below, the nipple. The heart's action is variable, trembling and irregular at times. The aortic sounds are both very indistinct, but with a binaural stethoscope I think that there are both an obstructive and a regurgitant murmur. The second sound is certainly louder over the pulmonary artery. The pulse in both radials is visible and jerky, and especially so on the right side. Temperature normal. Sensation is greatly decreased or abolished on the paralysed side. When the left leg is pricked with a needle, the patient denies feeling it; but brings up the right leg to rub the part, or to lift it out of the way. A prick of the needle is at once localised on the right side. The left leg makes some slight unconscious movements to escape irritation.

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Rigidity of the limbs is much less marked than at first. The face has a very waxen appearance, especially on the paralysed side, which is polished and glossy. Crumbs of bread stick, after eating, to the paralysed half of the lips.

14th.—The milk having become scanty, the child was removed from the breasts. The pulse is now quicker and weaker (104), and the evening temperature is 101·4°. Rigidity has quite gone. Complains of constant pain in the left ankle and up the left leg. Condition otherwise unaltered.

17th.—There has been a decided rise in the temperature for the last three days; the evening temperature varying from 100·5° to 102·5°, with slight morning remissions. The pulse is weaker and faster than at first, and the respirations have become somewhat quickened and laboured. Appetite bad, and she is obviously losing ground. Quiet during the day, but considerable raving at night.

22nd.—Patient died this evening; the heart's action, for six hours previously, having been very irregular and weak. During this time she complained a great deal of severe angina-like pains, caused, she stated, by the heart's action. Respiration difficult. Semi-comatose before death, with lividity of face and extremities.

The *post mortem* examination was made eighteen hours after death, by the late Dr. Harvey, whose report was as follows:—

Rigor mortis slight. Breasts flaccid, still containing a good deal of milk. Uterus pale and well contracted, almost to its normal size. Peritoneal cavity normal. Kidneys and liver healthy. Spleen pulpy and enlarged. Pleural cavities healthy, except for some slight old adhesions. Lungs well collapsed. Slight excess of pale straw-coloured fluid in pericardium. Heart large and thickly enveloped in fat. Right auricle large, and contains a large-sized clot. In the right auricular appendix there is a clot, which is probably ante-mortem. Right ventricle large, with very pale muscular fibre. Left auricle not very large, with an indistinctly ante-mortem clot in its appendix. Left ventricle contains a similar clot, more characteristically ante-mortem. Its muscular walls are somewhat hypertrophied, and better coloured than those of the other cavities. Water poured into aorta flows gradually into ventricle, through the valves. Slight commencing atheroma of the valves. One of the semi-lunar valves presents a small conical vegetation, with vascular and abraded apex, evidently of recent origin. Other valves healthy. Lungs healthy, excepting slight emphysema. On opening the dura-mater a considerable amount

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of semi-turbid fluid is found upon both sides, more especially beneath the arachnoid. The right carotid, cut across in removing the brain, is found to be occluded by an embolus, extending some distance along middle cerebral artery. The middle lobe of the right side is much softened, and some slight amount of purulent-looking fluid is found in the lateral ventricle of the same side. The optic thalamus and corpus striatum are soft and pulpy upon the same side, markedly contrasting with their healthy condition upon the left side.

I should add to the above history of this case that this patient had never, so far as I could ascertain, complained of any symptoms specially referable to the heart during her pregnancy, and that she had never suffered from rheumatic fever or any of the more usual causes of heart trouble.

The case which I have thus recorded, from notes taken at the time, raises, I think, some very interesting questions upon which much has not yet been written—the ætiological relations of endocarditis and of certain forms of paralysis occurring during pregnancy or the puerperal state. It is all the more worthy of record in consequence of the *post mortem* examination having been made by so well-known an authority as the late Dr. Reuben Harvey.

I purpose briefly considering, firstly, the connexion, if any, existing between endocarditis and pregnancy, including the puerperal state; and, secondly, the causation of what has sometimes been called "puerperal hemiplegia."

The conditions of the blood, and vascular system generally, of the pregnant woman, tend to establish an *à priori* probability in favour of the causative or predisposing influence of pregnancy in regard to endocarditis. These conditions, which may be assumed as being well established, consist in (1) an increased quantity but deteriorated quality of the blood; (2) an hypertrophy of the heart, especially of its left ventricle; and (3) an increased blood tension. The total mass of the blood is largely increased in quantity during pregnancy, a fact chiefly due to an increase of its watery part. The red globules are notably decreased, and the white ones increased

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in proportion to the amount of fluid; and from the sixth month onwards there is a marked increase in the proportion of fibrin. It must be further remembered that shortly after delivery we have the blood stream further adulterated by the discharge into it of much of the waste stuffs of the disintegrative processes attending involution.

The hypertrophy of the heart, now generally admitted, though its constancy is by some denied, is simply an example of physiological adjustment to increased work. The increase in work is necessitated by the formation of the utero-placental arterial and venous system, as well as by the increased volume of the blood and heightened arterial tension. The heart also beats more frequently than normally, so that in every way its work is increased.

Sir J. Simpson, in 1854, was among the first who suggested the influence of pregnancy upon the genesis of endocarditis. Having indicated the general connexion existing between endocarditis and diseases attended by a vitiated condition of the blood, he argues thus:—"We cannot wonder that similar lesions should occasionally occur in the puerperal female, for the condition of her blood is temporarily changed even under normal circumstances, and often becomes much and morbidly changed under the abnormal circumstances to which she is then liable to be exposed, so that its characters are then more or less assimilated to those of the same fluid in the subjects of acute rheumatism and chronic albuminuria." Again: "During the puerperal condition the blood is more loaded with new materials—intended, some for secretion and some for excretion—than at any other term of life, and hence is specially liable to diseased changes under the superaddition of any exciting or septic cause." And in the same essays he pointed out quite clearly how the hyperfibrinous state of the blood before or after delivery predisposed to the formation of loosely adherent coagula upon the surface of any old endocardial roughnesses.

The five examples of puerperal endocarditis given by Sir J. Simpson were probably all cases of acute vegetative, grafted upon a previously chronic endocarditis; and one of them is further specially interesting, as having been associated, as in the case I

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have recorded, with hemiplegia. I do not wish, however, to delay over the dangers which, since Simpson's time, have been universally accepted as pertaining to old endocarditis during pregnancy. It is sufficient to point out that they consist largely in the tendency of such chronic inflammations to become intensified and acute, owing to conditions of the blood and vascular apparatus, which must be accepted as likewise tending towards the development *de novo* of endocardial inflammations at such times.

But beside this recrudescence of an old inflammation there is another form of endocarditis, universally acknowledged to arise, in some cases, as a result of pregnancy, and still more frequently shortly after delivery. It may then arise *de novo*, or graft itself upon a pre-existing endocarditis, and it may originate from a puerperal as from any other pyæmia, or spontaneously, so as to give rise of itself to a typhoid or pyæmic condition, marked by numerous small metastatic abscesses. Its attack in a previously healthy subject is often signalised by a rigor, and the true cause not being suspected, the disease is mistaken for some other acute disease. It is by no means improbable that some of the rare examples of the so-called "puerperal fever," arising during the last weeks of pregnancy, without any recognisable means of utero-infection, may really have been cases of this quickly fatal form of endocarditis. It is almost certain, however, that this is really a septic endocarditis, though the source of the organisms (micrococci) cannot in many cases be explained. The micrococci of this disease do not propagate in the blood of the patient. They are carried in minute embolisms from their cardiac source, and seem to give rise to metastatic abscesses by the virulent properties of their chemical products when inoculated. They multiply locally in the tissues of other animals (rabbits) and give rise to a very intense inflammation, What I desire especially to point out as regards this disease is, that it may arise suddenly and spontaneously during pregnancy or the puerperal state without the necessary pre-existence of any pyæmic condition. A very striking fact in reference to this form of puerperal endocarditis is alluded to by Lancereaux (A. G. de M., Vol. I., 1881, p. 402), viz., that it is specially likely to occur in

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patients whose nervous systems are for some reason depressed. Thus he has collected seven cases, in six of which the patients were unmarried primiparæ. To this fact we have a remarkable analogy in the well-known predisposition of unmarried primiparæ to suffer from the various kinds of septic diseases of childbed.

We have seen that pregnancy is capable of starting fresh developments of inflammation in cases of old endocarditis, as also that it may originate a malignant form of endocardial inflammation. It has also been affirmed by some that it may constitute a true exciting cause to subacute or chronic valvular disease. Whether or no this is true—as it may appear to have been so in the case recorded—I am not prepared to say upon the evidence I have been enabled to collect from various sources. Although in my case the opinion of Dr. Harvey seems amply sufficient to prove that the aortic lesion was of quite recent origin, yet it must be remembered that the patient had suffered unusual hardship and privations during her pregnancy. And endocarditis has been known to arise under such circumstances, apart altogether from pregnancy or any of its more generally recognised causes. This is just one of those cases in which we might, with some show of reason, fall into the error of mistaking a coincidence for an effect. Without wishing to give any decided opinion upon this question, I may say that the evidence I have been able to collect appears insufficient to establish a true *causal nexus* between pregnancy and endocarditis other than ulceration attacking previously healthy valves.

The occurrence of various paralytic affections during pregnancy and the puerperal state has long been recognised and explained by different hypotheses—*e.g.*, hysteria, direct pressure upon the pelvic nerves, albuminuria, "apoplexies," &c. It is only comparatively recently that the origin of the most common of these paralyses—viz., hemiplegia, has been proved by accurate *post mortem* examinations to depend upon organic lesions of the brain, resulting from embolisms of various kinds. Simpson, in his papers upon puerperal arterial obstruction, has recorded two cases of hemiplegia resulting from organic lesions of the brain caused in this way; and Ollivier

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has also recorded an example of what was obviously an embolic hemiplegia. In 1879 Fordyce Barker recorded three cases of hemiplegia occurring in puerperæ, and due to embolism. In one of these cases the hemiplegia supervened suddenly during the first stage of labour in a previously healthy primipara, aged thirty-two. The patient recovered, but the paralysis, though lessened, remained chronic. In Dr. Barker's other cases the patients died, but the nature of the cardiac lesion, the fever and delirium during life, and the numerous infarcts of other organs than the brain found after death, seem to show that these were cases of ulcerative endocarditis. In one a subacute rheumatism appears to have been present. It is further noteworthy, that in these two cases of ulcerative endocarditis, recorded by Fordyce Barker, the patients were unmarried, a fact which supports Lancereaux's observation already noted. In one of the latter's cases also an embolism is recorded as having obliterated the left Sylvian artery.

In February, 1881, Dr. Hughes Bennett published in the *British Medical Journal* a clinical lecture upon chronic hemiplegia, originating during the puerperal state. He records six cases of this affection which came under his notice during three years in hospital practice. In only one of these cases did there exist, at the period of their coming under his notice—at from eight weeks to eight years after the accident—any distinct evidence of cardiac affection. They all occurred in rather young women, averaging 25 years; and this fact is regarded by Dr. Bennett as decisive against the cause having been hæmorrhage. The permanence of the brain lesion is likewise against the theory that they were due to albuminuria, which was moreover absent at the time they came under notice. He sums up his conclusions as follows:—1. These six cases of hemiplegia are due to a destructive lesion of that part of the brain supplied by the middle cerebral artery. 2. The disease began suddenly during the puerperal state, which was the predisposing cause of it. 3. The exciting cause was probably an embolus. 4. This embolus was the result of acute endocarditis, or due to the hyperinotic state of the blood, or possibly to both of these conditions combined. All Dr. Bennett's cases being still

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alive, there was of course no possibility of certainly verifying his conclusions.

In many of the cases of embolic hemiplegia previously recorded, and thus verified, the lesion appears to have arisen in consequence of the ulcerative form of endocarditis. In that, however, which I have now recorded, the history of the case and the *post mortem* appearances prove with sufficient clearness that the embolus arose in consequence of an acute or subacute simple endocarditis, originating during the latter months of pregnancy, and affecting one only, and that an aortic valve. Death seems to have resulted from the failure of a fatty heart to carry on the circulation.

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ON THE TREATMENT OF UTERINE FIBRO-MYOMATA.

By THOMAS MORE MADDEN, M.D., F.R.C.S.E.;
Obstetric Physician to the Mater Misericordiæ Hospital;
Physician to the Hospital for Sick Children, &c.

[Read in the Obstetrical Section, March 6, 1885.]

The prominence into which abdominal section has recently come in the treatment of uterine tumours by hysterectomy, oöphorectomy, and other intra-peritoneal operations, unquestionably deserves the special consideration of our division of the Academy of Medicine. I therefore submit the following observations on the treatment of myomata by those various methods, old and new, of which I have had experience, mainly in the hope of inducing the discussion of a subject of as great practical importance as any that could engage our attention.

The progress of gynæcological surgery during the past decade is well exemplified not only in the improved treatment of intra-uterine tumours, but still more so in the bolder operations by which even the largest sub-peritoneal and intramural fibromata may now be removed, or arrested in their development.

These objects are attainable either *per vias naturales* or through abdominal section. In the former method are included removal by écrasement, thermo- and galvanic cautery, hemp saw, bistoury, polyptome, or curved scissors, enucleation, and traction, as well as vaginal hysterectomy, and oöphorectomy. Amongst the latter are abdominal hysterectomy, myotomy, and oöphorectomy.

The special applicability of these procedures or of their various modifications, must be mainly determined by the physical character

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and position of the neoplasm, and the general condition of the patient in each case.

In the first place we shall refer to the curative results obtainable from some of the more generally indicated and safer intra-uterine methods of removing fibro-myomata. Secondly, the special advantages and special risks of the treatment of these tumours by laparotomy, hysterectomy, myotomy, and oöphorectomy, will be considered.

As I am unable to illustrate these observations in the manner which has recently become popular, by referring to my cases in series of thousands, and as any lesser number would, I presume, be now regarded as beneath notice, I shall not attempt here a statistical summary of my experience. But I may venture to observe that it cannot be very slight, inasmuch as it extends over a period of nearly seventeen years of gynæcological work, during each year of which I have almost daily had to deal with cases of fibro-myoma in hospital, dispensary, and private practice.

In the vast majority of instances the uterine tumours that come under our observation are submucous, and more frequently pedunculated, or semi-pedunculated fibro-myomata. The latter, whether intra-uterine or extending into the vagina—or, in other words, polypoidal—may be simply removed by the simplest of all operations—namely, the division of the pedicle, either by écraseur, polyptome, bistoury, or curved scissors, and subsequent extraction *per vias naturales*.

Uterine Dilatation.—Before, however, any intra-uterine surgical treatment can be attempted in such cases, whether by écrasement or by enucleation, the cervical canal and cavity of the uterus, if not already sufficiently patulous, must be so expanded by mechanical means as to allow of the necessary local manipulations. For this purpose an ancient but long disused method of gradual dilatation with sponge tents was reintroduced into practice by Sir James Simpson, and down to the time of the late Dr. Marion Sims no better way of opening the uterine cavity for examination or treatment than the use of sponge, laminaria, or other equally slowly expanding tents, was known. So many, however,

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were the objections to all these that it must be admitted that few greater practical improvements have been effected in recent intra-uterine surgery than the method of rapid cervical dilatation now available either by the use of Hegar's or Lawson Tait's dilators, or by the skilled finger of the surgeon, which is, I believe, by far the best of all instruments for this purpose. Rapid dilatation is especially applicable in the case of intra-uterine tumours occurring in pluriparæ, whose uterine contractibility has been further diminished by local hæmorrhage consequent on the neoplasm. In such cases the uterine cavity may generally be readily and rapidly laid open by putting the patient under the influence of anæsthetics, then firmly but gently introducing our right index into the cervical canal, and afterwards crowding in finger after finger until the canal is rendered completely patulous.

In some instances, however—more particularly in the case of sterile patients, in whom the cervix has not been taken up by the development of the tumour—immediate dilatation may be quite impossible; in such cases, therefore, we must still avail ourselves of expansion by the simultaneous use of a number of sea-tangle bougies.

Écrasement.—Any submucous fibro-myoma which has either become pedunculated, or which protrudes so far into the uterine cavity as to afford possibility of encirclement at its point of projection from the uterine wall, may be removed by écrasement with a strong steel or piano wire—the superiority of which over any form of chain or wire rope, or other method of dividing the pedicle, has been demonstrated by Dr. Kidd, whose procedure in this respect I have followed in many instances.

Enucleation.—This operation, although usually restricted to submucous tumours, is, in my opinion, applicable to fibro-myomata, whether submucous or more deeply imbedded in the uterine parenchyma, which, from the position and size of the neoplasm, are accessible and capable of extraction *per vias naturales*. If this view should be accepted, it would obviously provide an alternative and probably a safer operation in some of the cases in which hysterectomy and other intra-peritoneal operations are now advo-

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cated. Every myoma is, primarily at least, histologically undistinguishable from the uterine structure in which it originates, and is converted into a fibro-myoma or fibroid only by the gradual development of its connective or fibrous tissue. Before this process is accomplished—and we are seldom consulted sooner—the tumour also, as a general rule (to which, however, there are many exceptions), becomes encapsuled, or distinctly separated by an intervening layer of cellular tissue from the uterine parenchyma in which, as already said, it originated, and from which, however deeply imbedded, it can in most cases be shelled out and removed, or enucleated.

The operation by which this may be accomplished is, in practised hands, a comparatively simple matter. In the first place, as for écrasement, the cervical canal must be previously dilated, the patient placed in a semi-prone lateral position and etherised. Next, the uterus should be washed out with a warm carbolised solution, so as to diminish its vascularity as well as to render it as far aseptic as possible. Then a free incision may be made through the endo-uterine mucous membrane and capsule into the most prominent part of the tumour. This is now seized with a strong vulsellum, by which firm traction is made downwards in the direction of the pelvic outlet, whilst at the same time with the operator's finger—or where this cannot reach, then with either a curved silver spatula or with Thomas's spoon forceps—all adhesions around the tumour are broken up. Lastly, the fibroid by traction with the vulsellum from below, aided by firm pressure from above, is forced out of its bed and extracted either with the forceps or by splitting into sections that may readily pass through the vulva at outlet. By this operation I have repeatedly removed not only large submucous fibro-myomata, but also interstitial, or in some instances partially subserous, tumours. As, however, the feasibility of this procedure in the treatment of any deep-seated mural or partially sub-peritoneal fibroid is not generally recognised, I may here cite a somewhat remarkable instance of this kind in proof of the occasional curability of such cases, even under very untoward circumstances, without either abdominal section or vaginal hysterectomy.

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An unmarried woman, aged thirty-eight, who, until the day of her reception into the hospital, had been able to follow her avocation as cook in a large hotel, was admitted under my care. For the preceding two years she had been complaining of continual pain in the back and sense of pelvic weight, debility, leucorrhœa, and slight menorrhagia. These symptoms had gradually increased, but at no time was the menorrhagia very urgent, the changes merely lasting five or six days, and returning every three weeks; nor was there any hæmorrhagic discharge in the interval. On admission her chief suffering was from distressing dysuria—so great that she had been obliged for some time to visit a medical practitioner every day to have a catheter passed. She also suffered from frequently-repeated and generally futile calls to defecation. She had in addition to tenesmus slight prolapsus ani. Her feet and legs were œdematous, and she complained of great pain along the course of the left sciatic nerve.

The vagina was small and the hymen unruptured. On recto-vaginal examination the uterus was apparently completely retroflexed, the hollow of the sacrum being occupied by a large globular tumour extending up as high as the finger could reach and pressing downwards into Douglas's space. On examination with the sound, which passed in upwards of six inches anteriorly, it was evident that the double compression of the rectum and neck of the bladder was caused by a uterine tumour. To discover the position of this growth, the cervix was packed with five sea-tangle tents. On the removal of these next morning the uterine cavity was fully laid open, and as the tumour was interstitial, it was necessary to dissect it out from the posterior wall, in which it was situated, and where it had developed outwards, so as have become in great measure sub-peritoneal, the posterior surface of the growth being covered merely by a thin capsule of the uterine structure. In the separation of the tumour, which was as large as the fœtal head at the seventh month, the uterine tissue—which had become disorganised by the pressure of the morbid growth, and was, as usual in such cases, extremely soft and friable—was unavoidably lacerated. This rent extended downwards and backwards through the out-

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stretched roof of the posterior vaginal *cul-de-sac*, and left a wide opening into the abdominal cavity. As soon as the tumour now shown (which, as may be seen, was as large as the fœtal head at the seventh month) was extricated, which had to be effected by the midwifery forceps, a large coil of intestines came down, filling the vagina. These were immediately returned, and the patient being then in such a condition of collapse that no attempt could be made to close the laceration by sutures, it was merely plugged with a large sponge, so as to prevent for the moment any further prolapse of the intestines. A drachm of ether was injected hypodermically, and a little brandy and tincture of opium thrown into the rectum. Her pulse, which had been almost imperceptible, became a little stronger, and she was removed to bed and there surrounded with hot jars, &c., with the faintest hope of reaction. This, however, took place, and a couple of hours later her pulse was fairly recognisable, and her aspect improved.

On the second day severe metro-peritonitis set in, and for seven or eight days afterwards her life hung in the balance. It is needless here to dwell on the treatment pursued, which consisted mainly of opium and small doses of mercury, hydrocyanic acid draughts, &c., with the usual local applications—namely, leeching on two occasions, and continual use of fomentations or anodyne poultices to the abdomen, together with warm antiseptic vaginal injections. The vagina was plugged with sponges wrung out of weak carbolic solution. The daily changes of these were effected under carbolic spray, an atmosphere of which was maintained about her. For some days she suffered from incessant retching and hiccough, which were controlled by hydrocyanic acid and ice. By the mouth she was allowed only iced champagne in very homœopathic doses, her nourishment for ten days being enemata of beef extract with a little brandy and arrowroot. It is unnecessary to follow the daily notes of the case further than to say that after the subsidence of the peritonitis, from which she was not free for many days, it was found requisite to continue the vaginal plugging for another week, when sufficient adhesion was formed to allow its discontinuance. After the operation her bowels were kept confined for as long as

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possible. At the end of three weeks she was able to sit up, and a week later was sent to the Convalescent Home at Stillorgan, whence she returned to her former occupation, and is now again employed at the hotel from which she was sent to the hospital.

It is hardly necessary to observe that the enucleation of any fibroid, especially of one deeply intra-mural, is never devoid of considerable danger. But this danger is, we believe, less than that of any other of the operations which are more generally sanctioned for the same purpose. The risks of enucleation are—firstly, that, as happened in the case referred to, the tumour may have so thinned out the uterine wall behind it that this may be ruptured during the operation, and thus probably cause immediate death from shock or hæmorrhage, or subsequently from metro-peritonitis or septicæmia. Even where the integrity of the uterine wall was not affected, I have seen death from the latter cause follow the enucleation of a large fibroid.

Hence, it would be impossible to lay too much stress on the necessity of strict antisepsis, not only during the operation itself, which should always be Listerian, but also in the after-treatment by carbolised injections, &c., until the uterine wound has become sealed. Nor, in my opinion, should the operation be ever undertaken by any surgeon who does not believe in the efficacy of the antiseptic system, and who has not time and patience for personally carrying out the subsequent treatment of the wounded uterus in accordance therewith.

Removal by Traction.—In the case of those deeply-imbedded myomata which are not encapsuled we may, in some instances, as shown by the specimen that I have exhibited, succeed in their removal by the operation which is recommended by Dr. Emmet, not only in these cases but also in the treatment of other fibro-myomata, in which I would myself prefer to attempt enucleation. The object of Dr. Emmet's operation, which he terms "removal by traction," is the immediate conversion of the tumour by tractile force from an intra-mural, or sessile, into an intra-uterine pedunculated, or polypoidal, form. With this view, says Dr. Emmet:—"For the relief of these cases it has been my practice to excite

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uterine contraction by making traction on the growth in the direction of the uterine outlet. This action I have continued until the tumour becomes pedunculated, from being crowded out of its bed by muscular contraction closing in around and behind the mass.

"I recommend as a principle of practice to delay all surgical interference as long as possible. But as soon as the tumour presents at the os, and this begins to dilate, we then have proof that a reasonable amount of uterine muscular tissue remains to aid us. As soon as the vagina is occupied by a portion of the tumour, the operation for its removal cannot be long delayed, for it becomes then a question of but a few hours, as a rule, before blood-poisoning may supervene.

"Whenever the operation has been once commenced, there is but one course to follow, in removing the entire tumour, as the one attended with the least evil and risk to the patient. Whenever the tumour can be forced out by uterine contraction as rapidly as it can be removed at the vaginal outlet, the operation will be attended with but little risk of life. In my experience, so far, there has been no greater disturbance than that attending any ordinary case of instrumental labour whenever the tumour has been brought down to a pedicle and then divided. Our purpose is at first to excite uterine contraction by traction on the tumour, and this stimulant is maintained as it is being removed piece by piece from the vagina. As there is no fear from hæmorrhage, since the supply of blood is cut off as soon as the uterus begins to contract, our best means for removing the tumour is by a pair of blunt-pointed scissors, curved somewhat on the flat side. The operation is best begun by passing a slip-knot high up around the mass, which is to be held by an assistant, to steady the uterus, and for making traction. After having removed the portion which first filled the vagina, it is best to follow afterwards, as far as possible, the course of the uterine canal. The advantage is twofold, since the portion projecting into the canal, with the capsule-like covering, is firmer, and by removing first the tumour at the most distant point, the line of attachment becomes narrowed as the uterine cavity can be lessened.

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"When the tumour has been removed, with all shreds or loose portions within reach, it is important to wash out the cavity thoroughly. It is best to use very hot water, for it is a prompt exciter of uterine action, and by prolonging the injection we can thoroughly empty all the capillaries within reach of its direct influence."

I have here quoted Dr. Emmet's account of his operation at some length, inasmuch as its results have been most successful in his own hands and in the practice of others who have closely followed his directions for its performance. Moreover, its results have been thus favourable in many instances in which the character or position of the tumour would render its removal by enucleation impracticable; but in the case of distinctly encapsuled fibro-myomata, more especially if submucous, I still think enucleation a more generally feasible and a safer operation.

Abdominal Hysterectomy—Myotomy—Oöphorectomy.—Abdominal section has now come into such use in the treatment of uterine tumours, whether for their removal by hysterectomy and myotomy, or for the arrest of their development by oöphorectomy, as to require special consideration. The favour with which these intra-peritoneal operations are regarded by some surgeons has mainly arisen from the successful results of ovariotomy in modern practice; but whether the parity of treatment suggested in some instances by the symptomatic resemblances between uterine neoplasms, especially fibro-cystic tumours and ovarian growths, from which they are pathologically so dissimilar, is practically justified or not, is one of the most important questions that can engage our attention. Nor, whilst availing ourselves of the recent experience of those who may be regarded as pioneers in a still unsettled region of gynæcology, can we unhesitatingly adopt the too hasty generalisations founded on data so one-sided and as yet necessarily imperfect. We may therefore briefly consider the results of abdominal operations in the case of fibromata generally, and then refer more particularly to the various procedures included under this heading.

In the first place, Mr. Lawson Tait's recent address on one thousand abdominal sections must be referred to; for, whether we

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agree or disagree with Mr. Tait's views, we must all recognise his special experience and operative skill, as well as the unrivalled pertinacity with which he has maintained his convictions in spite of the disapproval of many other specialists in this branch of surgical practice.

Amongst the cases reported on by Mr. Tait are included the removal of the uterine appendages for myoma in 99 instances, with 7 deaths; hysterectomy, in 54 cases, with 19 deaths; and one enucleation of a myoma which proved fatal. Thus we have in all 124 completed abdominal operations bearing immediately on our present subject. From the same statistics it may be gathered that Mr. Tait has, moreover, had no less than 30 incomplete operations of this kind—17 of which were in cases of uterine, or unspecified but non-ovarian, tumours, in which, after the opening of the abdominal cavity, further procedure had to be abandoned. Of these incompleted operations Mr. Lawson Tait thinks that he "may speak with a certain amount of satisfaction," though from what he derives this contentment I am at a loss to understand, as his mortality in them was 50 per cent.:—"This mortality," he naively observes, "is of course heavy, and the results in the great majority of those who survived the operation were very unsatisfactory, though in some the disease has been arrested apparently for an indefinite time. I have no doubt now that in many of those cases I might have finished the operation—in fact, I know I could, but I always had a horror of a patient dying on the operating table, and from that distressing incident I have hitherto been entirely free. I now think that it would have been better even to have had such a disaster, and to have finished a large number of these operations."

The journal from which I have just quoted contains other evidence of the spreading cacoethes operandi, prevalent amongst abdominal sectionists of the Birmingham School. Thus Dr. Savage, in reporting upwards of a hundred cases of abdominal section, undertaken within a year, boasts that he performs this operation in every case which he has "the opportunity of operating on," without "the slightest attempt at the selection of cases, and as choosing the most suitable and rejecting those which did not seem

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to promise to be successful." Nor does he hesitate to admit that, had there been such selection of cases, he would have had fewer deaths of patients on whom he "operated with the idea of giving them the slight chance of life the operation afforded, knowing well beforehand how slight that chance was."

Dr. Keith has recently recorded thirty-eight hysterectomies with only three deaths. In the *American Journal of Obstetrics*, Dr. Bigelow has, with great research, collected from all available sources 359 similar operations, of which 227 resulted successfully, whilst 132 patients died. With regard to this admitted mortality, greater than one in every three operated on, we may well ask ourselves the question which was suggested to Dr. Keith by his far more successful practice, viz.:—"Does a mortality of 8 per cent. justify an operation for a disease that, as a rule, has only a limited active life, that torments simply, and that only for a time, though of itself it rarely kills? The mortality of an ordinary uterine fibroid, if left alone, is nothing approaching a death-rate of 8 per cent. Most of the cases on which I have operated were known to me for years before; only the extreme cases were done; in nearly all, the lives were useless, and the risk of operation was clearly understood. Considering the nature of the cases, it seems to me that these operations were, perhaps, justifiable; and, if these were barely justifiable, what can be said of those ghastly lists of hysterectomy where the mortality is one death in every two, one death in every three, or even one death in four or five."

Believing, as I do, that abdominal operations unquestionably afford the only possible method of saving life, or relieving otherwise irremediable suffering in some cases of uterine fibro-myomata, I trust that the voice of the profession will make itself heard in no uncertain tone in reprobation of the operative *furore* which has manifested itself in some quarters with regard to the performance of abdominal sections for uterine tumours, and which else may lead to a reaction against the legitimate employment of procedures so valuable in suitable cases. In my student days I was taught that an operation was justified only as means of saving life or relieving suffering otherwise hopeless. It would now, however, appear that

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the gravest operations may be resorted to in every case in which the opportunity presents itself, and without any regard to their probable consequences. For my own part, I am unable to accept this doctrine, and am still old-fashioned enough to believe that no operation—especially one of such gravity as that under consideration—should be undertaken save as a matter of necessity, and with a reasonable prospect of a successful result. If the indiscriminate performance of gastrotomy now advocated by some eminent specialists be unfortunately ever generally adopted, then the sooner the better it will be that those suffering from any of the diseases in which abdominal section is thus abused should abandon surgical aid and trust themselves, as formerly, to unassisted Nature's kindlier euthanasia.

We may now briefly consider the circumstances under which abdominal section may be legitimately employed in the treatment of uterine tumours, and the operations which may by this procedure be resorted to in such cases.

Hysterectomy.—Abdominal hysterectomy, or the abstraction of the uterus by gastrotomy, was first successfully performed for the removal of a uterine tumour, forty years ago, by the late Dr. Clay, of Manchester, and shortly afterwards was again undertaken, in a similar case and with similar result, by Dr. Atlee, of Philadelphia. It was not, however, until long subsequently, when MM. Hardy and Pean, of Paris, in 1873, published their report of nine hysterectomies—seven of which were successful—that this procedure was recognised as a legitimate procedure. In the same year it was first brought before the profession in these countries by Mr. Lawson Tait, who, in a communication to the Medical and Chirurgical Society of London, narrated a case in which he successfully removed a uterine fibroma, weighing eleven pounds. From that time hysterectomy has gradually but surely come into yearly-increasing favour as a means of treating cases of fibromata otherwise beyond remedy. Of late, however, this operation has been extended far beyond this, and by some specialists has been recently pushed to an extent and applied to cases where no absolute necessity exists for resorting to its performance, and it cannot, in

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my opinion, be justified by any statistical account of its success yet reported.

The removal of the uterus for fibro-myomata may be performed either by abdominal section or per vaginam. The former method being unquestionably the more feasible, and the best adapted in the case of those large fibroid or fibro-cystic growths in the treatment of which the ablation of the uterus may in some instances be necessitated, will alone be here referred to.

Abdominal hysterectomy is usually performed in the manner originally suggested by Dr. Freund, of Breslau, whose directions for its accomplishment, since somewhat modified by Dr. Bantock, Sir Spencer Wells, Mr. Lawson Tait, Dr. Agnew, of Philadelphia, and other recent operators, have been adopted in the cases that have come within my own observation.

Myotomy.—According to Professor Schroeder, any uterine fibro-myomatous tumour, however extensive or wherever situated, may be removed abdominally by laparotomy or partial hysterectomy with the aid of the elastic ligature. The appalling mortality resulting from this operation should, however, I think, sufficiently prevent its repetition by other surgeons. Of those on whom Schroeder thus operated he lost in his first series of cases 30 per cent., and in his second series 22 per cent. of his patients.

Oöphorectomy.—The removal of the uterine appendages, as originally suggested by Blundell, and reintroduced into modern practice by Dr. Batty, of Georgia, whose name as well as that of Dr. Goodell, of Philadelphia, is now identified with this operation, has been largely employed within the past few years for preventing the development of fibromata, and for the prevention of uterine hæmorrhage, consequent on their existence.

If we merely compare the general results of oöphorectomy with those of hysterectomy in the treatment of fibro-myomata as established by the experience of Knowsley Thornton, Lawson Tait, or Keith, there can be no question of the greater advantages of the former in the case of small uterine tumours. These advantages have been thus urged by Mr. Thornton:—"I believe," he says, "that there is a great future before this operation of removal

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of the appendages for the cure of fibroid—a future so free from mortality and so satisfactory in its after-results that we shall eventually be justified in performing the operation in a far larger number of cases than we could ever have hoped to justify the more dangerous operations which we have been considering. I feel sure that, with a little more experience, we shall be justified in recommending the removal of the appendages in cases which are troublesome from hæmorrhage, pain, &c., before they grow large enough to raise the question of the more dangerous operations."

Before, however, we can accept oöphorectomy as the panacea that it claims to be for uterine fibromata, it seems to me that further proof is still needed that it is either generally necessary, safe, or feasible in such cases generally. It would certainly, at least, be desirable to formulate, more distinctly than has been yet done, the cases of fibro-myomata in which oöphorectomy may be resorted to with a fair prospect of benefit, and to point out those in which no reasonable anticipation of success can be held out from its performance. In the first category should be placed generally all obviously actively increasing fibromata not removable *per vias naturales*, and more especially those occurring in young patients in whom the prospect of reaching the period when any arrest of the tumour by the natural menopause might be hoped for, is remote, and who, if they survive till then, are meanwhile necessarily condemned to lives of useless suffering. Under these circumstances there can be no question of the propriety of attempting by oöphorectomy to anticipate the distant menopause in any case in which this is feasible. But the removal of the ovaries as a general rule of practice, as now advocated merely for the arrest of hæmorrhage consequent on fibromata, appears to me unjustifiable until other and safer methods of checking metrorrhagia have been fully and unsuccessfully employed. And I am convinced by my own clinical experience that if we try these fairly we shall seldom find it impossible to arrest effectually and safely any uterine hæmorrhage thus caused without oöphorectomy. Secondly, I cannot think this operation generally advisable in the case of quiescent fibroids largely occupying the abdominal cavity in older patients. In such

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cases the removal of the uterine appendages is generally not merely difficult and hazardous, but even quite impossible in the instances in which, if practicable, it might be most useful. Thus in any large sub-peritoneal or interstitial fibroid lifting the uterus far above the pelvic cavity, and binding it to the adjoining parts by consequent inflammatory intra-peritoneal adhesions, it will be found utterly impossible to reach the ovaries by any abdominal section until the uterus by which they are overlaid and concealed is first detached from these adhesions and turned out of the abdominal cavity. In such a case, and it is no ideal one, having subjected our patient to all the risks of such an operation, are we to dissect out the uterine appendages and then replace the uterus and tumour *in situ*, or in the words of an eminent abdominal sectionist, Dr. Drysdell, of New York, would it not "be better practice to leave the uterine appendages untouched and remove the tumour itself?"

In conclusion, I append a few cases in illustration of the foregoing observations:—

CASE II.—*Recurrent Fibroid; Écrasement.*—K. N., aged twenty-eight, unmarried, was admitted January 4th. Two years previously she had been under my care in the hospital, suffering from menorrhagia. The uterine cavity was then dilated, and a small pedunculated myoma, growing from the anterior wall, removed. At the same time a second larger tumour was discovered deep within the posterior wall. It was not then considered advisable to interfere with this. The menorrhagia subsided in great measure, and she left the hospital. Some months later it again commenced, and for the past eighteen months she lost a considerable amount of blood, and when readmitted was in an extremely exhausted condition. The os was dilated in the usual way, and on examination next day we were pleased to find that the tumour, now as large as a fœtal head, had developed into the uterine cavity, and become pedunculated. The pedicle, which, as may be seen, was extremely thick, was divided by écrasement, and the tumour extracted. She made a good recovery after this operation.

CASE III.—*Submucous Fibro-Myoma removed by Écraseur.*—S. Q., a married woman, aged forty-four, who had repeated miscarriages but no family, was admitted into St. Monica's ward, January 14th,

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suffering from profuse metrorrhagia and frequent attacks of intense uterine colic. These symptoms had been first observed eighteen months before admission, and on examination a small tumour could be felt above the pubes. The uterus was enlarged and drawn up. The sound could not be passed in any direction more than an inch above the os internum, but on recto-abdominal examination the size and position of the uterus, which was slightly retroflexed, could readily be made out. Being placed under ether, rapid dilatation of the cervix was effected by Lawson Tait's dilators, and then by my fingers. The cavity of the uterus thus laid open was found distended by a large pedunculated submucous fibro-myoma, around which, as being tightly encircled by the uterine walls, the steel wire loop of the écraseur was with some difficulty slipped up to its attachment to the posterior wall, close to the fundus. The thick pedicle was then slowly divided, but with no amount of tractile force with two strong vulcella, in the hands of Mr. Kennedy and myself, could we succeed in extracting the freed tumour through the uterine outlet, until at last we were obliged to resort to a free bilateral division of the os and cervical zone, and in this way removed the tumour now shown. In doing so some disruption of the perinæum was necessarily produced. This was immediately treated by a couple of silver wire sutures. The after-treatment consisted mainly in hot antiseptic injections twice a day. Within a fortnight's time she was up and going about the ward, being perfectly convalescent after the operation. Subsequently, however, she was attacked by typhoid fever, of which she died.

Case IV.—*Intra-Mural Fibro-Myoma removed by Enucleation.*—L. B., an anæmic-looking woman, aged thirty-eight, unmarried, suffering from metrorrhagia, pelvic pain, and hæmorrhage, was admitted to St. Elizabeth's ward. Until two years previously she had been in good health. She then commenced to suffer from menorrhagia which gradually increased, and the intervals between the recurrence of the discharge became so diminished that for the past year she has seldom been a week free from hæmorrhage. On examination the uterus was found retroverted by a tumour in the posterior wall, and the uterine cavity greatly elongated. The cervical canal was dilated by laminaria bougies, on the removal of which she was etherised, and a submucous tumour discovered occupying the posterior wall from the fundus to near the cervix, and bulging out into the uterine cavity. A free longitudinal incision

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was made through the thinned muscular structure into the capsule of the tumour, which was firmly seized with a strong vulsellum and drawn down by Dr. Kennedy towards the outlet, whilst with my finger I rapidly separated the loose adhesions around and behind the tumour. This was now forced out of its bed and extracted per vaginam. Immediately afterwards a hot carbolised water injection was thrown up to arrest the free oozing, a tampon of Lawson's cotton saturated in carbolised glycerine was introduced, and an anodyne suppository placed in the rectum. The hot water injection was continued twice daily for the next ten days, at the end of which the uterus, being still large, was brushed out at intervals with tincture of iodine until it had nearly regained its normal size, and three weeks after the operation she was enabled to leave the hospital.

Case V.—*Interstitial Myoma removed by Emmet's Operation.*—In this instance I regret I can exhibit only portions of a myoma removed from a multipara aged forty-two. This patient had enjoyed good health until the birth of her last child, four years previously. Shortly after she became a widow. She then began to suffer from menorrhagia, tenesmus, and bladder irritation, and bearing-down sensation, which increased. At last she was forced to give up her occupation as a farmer's servant. On admission the uterus was found completely retroverted, and on endo-uterine exploration a considerable sized tumour was found bulging out into the uterine cavity. This I attempted to remove by enucleation, but on making an incision for this purpose it became obvious that the tumour was not separated by any capsule from the uterine wall, with which it was continuous. Hence, I resolved on trying to effect its removal, if possible, by Dr. Emmet's traction operation. For this purpose the most prominent portion of the tumour was firmly grasped by a vulsellum and forcibly dragged down through the os as far as possible into the vagina. Here as much of the growth as could be reached was cut away with a strong curved scissors. The resulting hæmorrhage was checked by hot water injections, and the remaining portion of the tumour again similarly treated. In this way we had removed more than two-thirds of the growth, when the patient became so collapsed that I was reluctantly obliged to postpone its complete ablation. A hypodermic injection of ether was administered and she was put back to bed. For the

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two days following her condition was apparently satisfactory, her pulse fairly good, and temperature not rising above 100°. On the second night after the operation she again became collapsed, and, despite the efforts made to save her, she sank and died.

Case VI.—*Pedunculated Submucous Fibroid removed by Écrasement.*—A widow, aged fifty, who had had several children, was admitted, suffering from continual uterine hæmorrhage and offensive leucorrhœal discharge. On examination the uterus was found occupied by a tumour as large as the fœtal head at full term. The os was rapidly dilated, and this growth, which proved a semi-pedunculated fibroid, was removed by écrasement in the usual way. After this she recovered rapidly.

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A CASE OF ARTIFICIAL VESICO-VAGINAL FISTULA FOR THE CURE OF CHRONIC CYSTITIS.

By ARTHUR V. MACAN, M.B.;
Master of the Rotunda Hospital.

[Read in the Obstetrical Section, April 10, 1885.]

THERE are probably few present who have not in their practice met with women suffering from chronic cystitis, to cure which all the ordinary remedies seemed powerless. That such cases, when uncomplicated with renal disease, are usually due to some form of septic infection of the mucous membrane of the bladder is, I think, nowadays hardly disputed, and the treatment that is found most successful—viz., the use of antiseptic injection—certainly supports this view. If these, however, fail we have it still in our power to aid and assist them by thorough drainage of the bladder. Some practitioners have sought to attain this end by leaving a catheter permanently in the bladder, but the objection to this plan of treatment is, that unless great care be used to disinfect the catheter it may itself produce decomposition of the urine, which leads in its turn to further cystitis. The most obvious and certain method of obtaining thorough drainage of the bladder is by making an artificial vesico-vaginal fistula near the neck of the bladder. As this operation has been but rarely resorted to in this country, I trust that the details of the following case, in which this treatment was adopted and proved eminently successful, may prove of interest to the members of this Section of the Academy:—

CASE.—Mary D., aged thirty-eight, was admitted to the Rotunda Hospital on March 4th, 1884, having been sent up by Dr. Stoney, of Abbeyleix. Was last confined on April 9th, 1879, being delivered by forceps, after a twenty-fours' labour. Immediately after

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delivery incontinence of urine set in. For this she came up to the Rotunda Hospital during the mastership of Dr. Atthill. He operated on her three times, and at the third operation turned the cervix uteri into the bladder. After this the woman went home, but she says there was still a leak of urine per vaginam.

Nearly five years after this she entered the Rotunda Hospital under my care, as she then had a vesico-vaginal fistula, and for about eight months had been suffering great pain in the bladder, with frequent discharge of blood. On making a vaginal examination a fistula was detected high up in the vagina, through which a sound could be passed into the bladder, on withdrawing which some bloody urine flowed into the vagina. On passing a sound per urethram a large stone was readily detected in the bladder.

On April 15th, 1884, I proceeded to remove the stone, in which operation I was kindly assisted by Dr. Corley. I first dilated the urethra to No. 18 Hegar, and introduced my finger into the bladder, and then making a bimanual examination easily felt the stone. This I got a firm hold of with a lithotomy forceps, but all my efforts to remove it through the dilated urethra failed. I then introduced an ordinary male lithotrite and endeavoured to seize the stone, but for a long time all my efforts failed from the bladder contracting tightly on the stone. At last I succeeded and removed several large pieces with the forceps, and then washed out the remainder with Bigelow's apparatus. The bladder was washed out daily after the operation, and shreds of mucous membrane came away for a considerable time; but the woman made a good recovery, and on May 2nd I operated on the vesico-vaginal fistula. The union was not, however, perfect, and so on June 5th I operated on her again, and this time successfully. Soon afterwards the symptoms of chronic cystitis became well marked and defied all treatment. The urine finally became so stinking that passing a catheter was a most unpleasant duty, and it contained shreds of the lining membrane of the bladder, just like small pieces of diphtheritic membrane. Having, therefore, exhausted all ordinary treatment, and the case growing gradually worse instead of better, I determined to try the effect of free drainage. With this view I proceeded on October 24th, 1884—more than six months after the operation for stone—to make an artificial vesico-vaginal fistula. A sound was passed into the bladder and its point made to project into the vagina at a point corresponding to the base of the bladder and on this the vesico-vaginal septum was divided. I then sewed the vaginal and

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vesical mucous membranes together. This I did with a twofold object—first, to prevent all hæmorrhage, and secondly, by covering over the raw surfaces with mucous membrane to try and prevent the vesico-vaginal fistula contracting and closing before the mucous membrane of the bladder had become healthy. On examining the wound two days afterwards the silk sutures were found thickly encrusted with the salts of the urine; and the mucous membrane of the vagina, from the seat of the fistula to the vulvæ, was of a brownish-yellow colour and thick and leathery. It was slightly separated round the edges, and on getting a firm hold of this loosened edge with a forceps I found I could peel the altered mucous membrane off the vaginal walls. This I now exhibit, and you will see that it has just the appearance of wet chamois leather. The surface left behind was raw and bleeding, but after a few days it became more healthy. After the operation the bladder was washed out daily with a weak antiseptic solution. This gave intense pain for a long time, small particles of diphtheritic-looking membrane being constantly detached from the interior of the bladder. By slow degrees the urine became more healthy, and these small particles of mucous membrane got scarce and finally quite disappeared, so that I looked on the cystitis as cured. I proceeded, therefore, on February 12th, 1885—or nearly four months after I had made the fistula—to close it again, and the woman has since been able to retain her water thoroughly. An attack of jaundice compels her still to remain in hospital, but the cystitis is thoroughly cured.

There is one point about the formation of the stone in this case to which I would like to draw attention. When there is a small fistula situated high up in the vagina the condition of the bladder seems to me to resemble very closely a cistern of water with an overflow pipe near the top. The urine accumulates in the bladder and finally reaches the level of the fistula, through which it escapes. The bladder is therefore never excited to contract and expel its contents per urethram, and the urine below the fistula stagnates and deposits its salts, which form a calculus. In a very similar case of vesico-vaginal fistula that has more recently come under my notice I ventured to say that we should probably find a calculus in the bladder, and my supposition proved to be correct.

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Since I operated on this woman I have adopted the same plan of treatment in a very remarkable case of cystitis, and with equally gratifying success. As, however, I intend bringing the latter case before the Academy at some future time, I will not enter into any of the particulars at present.

When considering afterwards the great difficulty experienced in this case in getting hold of the stone with the lithotrite and crushing it, and the unavoidable injury done to the walls of the bladder during the operation, and further the danger of the patient suffering from permanent incontinence of urine from over-distension of the urethra, I determined that in future I would remove any large stone by incision of the vesico-vaginal septum in preference to the method adopted on this occasion.

I have since had the opportunity of putting this resolution into action, and from my experience in these two cases I have no hesitation in saying that the method by incision of the vesico-vaginal wall and subsequent closure of the wound by sutures is infinitely preferable in all cases where the stone is of large size. The exact limit, where danger of permanent incontinence from dilatation of the urethra commences, was shown by the late Prof. Simon, of Hiedelberg, to be a diameter of 2 cm. Should the stone, therefore, exceed this limit, I would remove it by incision, rather than by crushing with a lithotrite.

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SOME POINTS IN THE DIAGNOSIS OF PELVIC HÆMATOCELE.

By WILLIAM J. SMYLY, M.D. Univ. Dub.; F.K.Q.C.P.I.;
Gynæcologist to the City of Dublin Hospital.

[Read in the Obstetrical Section, April 10, 1885.]

The term "pelvic hæmatocele" is generally understood to signify a tumour formed by the extravasation of blood, either into the peritoneal cavity or into the areolar tissue beneath; but by some authors it is restricted, and I think with advantage, to the intra-peritoneal effusions, the term "pelvic hæmatoma" being applied to all other forms.

Although nearly forty years have elapsed since this affection was accurately described, and though in that space of time an enormous literature has accumulated, yet we are not, even at the present moment, in a position to state whether its occurrence be frequent or the reverse. I have been informed by a gynæcologist of large experience that he had scarcely ever met with a case which he could pronounce, with absolute certainty, to be one of hæmatocele; whilst in the practice of another they appear to occur with remarkable frequency. This divergence of experience also pervades the literature of the subject. Thus, Hugenberger met with but two cases amongst 3,801 patients, and Scanzoni only eight during twenty-eight years' practice. In the Women's Hospital, New York, there were but four cases in twenty-five years, and in Dr. Emmet's private hospital but three. On the other hand, Seyfert, of Prague, reported 60 cases amongst 1,272 patients—equal to five per cent.; and Olshausen almost as large a proportion—namely, 29 out of 764. Dr. Emmet thinks that this varied experience "has been due to different classes of patients, and the different circumstances under which they were treated," and that physicians visiting patients at

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their own homes will see a larger proportion of cases than those whose practice is confined chiefly to the office, or to the wards of a hospital. The following statement of Bandl's, however, suggests another cause. He says:—"Out of 1,500 out-patients, examined during the last two years, we have been able to *demonstrate* retro-uterine hæmatocele in five, but in a much larger number of cases it seemed probable that tumours found posterior to the uterus had originated in blood extravasation." It appears probable that the inclusion, or exclusion, of such uncertain cases, partly accounts for the difference in statistics, and shows that, in spite of the advances made, accuracy in diagnosis is still, in a certain proportion of such cases, impossible. We must, therefore, hail with satisfaction any addition to our means of diagnosis, and put them to the test of practical experience. In the July number of the *Dublin Journal of Medical Science*, in 1884, I drew attention to some very interesting observations by Dr. Dick, of Berne, upon the diagnostic value of urobilin in the tissues and urine of patients suffering from extravasation of blood into the pelvic tissues. The application of the discoveries of Jaffé, Maly, Gerhardt, Bergmann, and others, to gynæcology, was original on the part of Dr. Dick, and has not, so far as I am aware, been confirmed by others. Urobilin is an amorphous, reddish-brown powder, possessing feebly acid characters, slightly soluble in water, but freely so in alcohol and alkaline solutions. Dilute alkaline solutions are the colour of ordinary urine, but on the addition of an acid become red. The addition of a few drops of chloride of zinc to an ammoniacal solution of the pigment, produces a most characteristic green fluorescence. Another valuable test is a remarkable absorption band between the green and blue in the spectrum.

The appearance of urine containing an excess of this colouring matter is so peculiar, that it at once suggests the idea of something abnormal in its contents. It varies from a light to a dark brown. In many cases there is a deposit of pigment in the skin and conjunctivæ, causing a brownish discoloration which has been called bilinicterus, to distinguish it from ordinary jaundice. In three cases reported by Dr. Dick, both the bilinicterus and urobilinuria were present. In the first two they were observed in patients in

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whom the extravasation of blood had been already diagnosticated, and were evidently the result of it; but in the third case it was their presence which led to the diagnosis of hæmatocele. This patient, aged thirty-five, had three children and one abortion, after which she suffered from pelvic inflammation. Having over-exerted herself at the menstrual period, she was suddenly seized with pains in the abdomen, which continued long after, but the flow did not come on. Dr. Dick first saw the case (in consultation) about five weeks after this. On examination, he found in Douglas' pouch a doughy, irregular tumour, which had displaced the uterus forwards. The skin was markedly yellow, and the urine, which was of a deep coffee-brown colour, contained a large quantity of urobilin. Not having seen the case from the commencement, and the history being indefinite, a certain diagnosis was not possible. However, from the large quantity of urobilin in the urine, and from the colour of the skin, he came to the conclusion that it was a hæmatocele. The tumour gradually reduced in size, and the icterus and urobilin also diminished. At the end of a month the tumour was aspirated, and dark brown blood was drawn off. For four days she went on well; then, however, the menstrual flow came on, attended by increased pains in the abdomen and enlargement of the tumour. Four days after this the icterus returned, and urobilin was again found in the urine. The tumour was again aspirated and a quantity of blood drawn off. After this, however, it rapidly increased in size, until it reached to the umbilicus, but this time there was neither icterus nor urobilinuria. As a rupture of the sac seemed to be imminent, an incision was made into it through the vagina, and a great quantity of putrid matter evacuated. The examining finger passed into a cavity, the walls of which were covered with old blood-clots. After this cavity had been irrigated with a 3 per cent. solution of carbolic acid, and a drainage tube introduced, the temperature rapidly fell. In this case it was certainly most remarkable that, when the tumour contained blood, there was urobilin in the urine; as the tumour diminished, so did the amount of urobilin; when it re-filled, the pigment re-appeared, both in the urine and in the skin; but on the third occasion, when it became distended with

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inflammatory and putrid fluids, there was neither icterus nor urobilinuria.

The following case, which I have had under observation, may prove of interest in this connexion. For the notes I am indebted to Mr. Poole, my clinical clerk:—

Case.—Mary C., aged thirty-one years, was admitted into the City of Dublin Hospital on the 15th of last September. She had had eleven children and one miscarriage. Her youngest child was sixteen months old, and menstruation had been regular up to the 9th of August, when she missed a period, and supposed herself to be pregnant. A fortnight after this she was seized with violent "cramps" in the hypogastric region, and retching, which continued for two days, after which there was a remission. Three days after she was again seized with such violent paroxysms of pain, that she rolled about the floor in agony. The abdomen became tympanitic and exquisitely tender; and there was much pain and difficulty in emptying the bladder and rectum, besides which she complained of a dreadful "bearing-down" sensation.

Condition on admission.—The patient presented a peculiar brownish yellow tint. Her pulse was 100 per minute, and her temperature normal. The lower portion of the abdomen was tender to the touch, but a tumour, which was dull upon percussion, and which reached nearly to the umbilicus, could easily be distinguished. This was discovered upon bi-manual examination to be the uterus, and the sound passed into it to a depth of three inches. It had been displaced upwards and forwards by a tumour which occupied the space between it and the rectum, and which bulged the posterior fornix downwards into the vagina, almost to the vulvar orifice. The surface of this tumour felt smooth to the touch, soft and elastic.

The night after this examination she suffered from crampy pains, which she said were like those attending a miscarriage, and some clots were expelled, but were unfortunately thrown out before they could be examined.

Next day the urine was drawn off with a catheter, but as it did not present anything abnormal in appearance, I thought it would be useless to have it tested for urobilin; the icterus, too, had faded considerably, and in two or three days more had entirely disappeared. From that time she made a rapid recovery;

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the tumour was absorbed all but a small lump about as large as a walnut, which can still be felt in Douglas' space, and the uterus returned to its normal position.

The presence of the icterus in this case was well marked, and had I been fortunate enough to have found the urobilin in the urine also, it would have been a valuable confirmation of Dr. Dick's observations. However, as it is, I hope the case will be of interest to the members of the Academy, and that, should examples of this affection come under their care, as no doubt they will, at an earlier stage they may detect these valuable symptoms.

That the case was one of hæmatocele I have no doubt—1st, from the history; 2nd, from the physical character of the tumour; and 3rd, from the rapidity of its absorption. That the extravasation was intra-peritoneal I gathered from the history—the patient being a married woman who had borne children, and who had not menstruated for upwards of six weeks before the accident—the physical characters of the tumour bulging downwards into the vagina, and its median position. Hæmatoma is, on the other hand, said to occur more frequently during menstruation. It is situated laterally in the pelvis, and often presents a flat or irregular surface towards the vagina, conforming to the base of the broad ligament.

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INTESTINAL OBSTRUCTION.

By RICHARD D. PUREFOY, M.D.;
Obstetric Surgeon to the Adelaide Hospital.

[Read in the Obstetrical Section, May 18, 1885.]

INTESTINAL obstruction, from any one of its various causes, must be regarded as a most dangerous accident common to all periods of life, and in infancy and childhood most frequently due to intussusception. We distinguish two kinds of invagination, differing greatly in their mode of origin and importance—namely, (1) the invagination of the death-struggle, which is an unimportant cadaveric incident causing no symptoms during life. It is accompanied by no imflammatory changes, and can be easily reduced. There are often two or more of them at the same time, mostly in the small intestine, never in the cæcum, and are ascending as well as descending; chiefly found in children, especially such as have died of brain disease. Secondly, the so-called inflammatory invagination, to which the following remarks solely apply.

It takes place, without exception, from above downwards, so that a portion of intestine is invaginated into another portion lying below it. The most common invagination is the ileo-cæcal—*i.e.*, the passage of the ileum and cæcum into the colon, preceded by the ileo-cæcal opening; at the apex of the intussusception lies the ileo-cæcal opening, and, close by it, the opening into the vermiform appendix, sometimes very much enlarged. In children, owing to the greater mobility of the colon in them, this invagination may in a short time pass through the colon down to the anus, or even through it. It was formerly supposed that the paralysed portion of the bowel formed the sheath of the invagination, but there are good reasons for thinking rather that the sheath is formed by a portion of the bowel still in a normal condition, and this best

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explains the clinical facts—viz., the sudden occurrence of violent and paroxysmal colic, tenesmus, and passage of blood.

Referring to its mechanism, Dr. Brinton says:—"The immediate cause of the displacement must be sought not only in abnormal violence, extent, and abruptness of the contraction in the transverse muscular layer, but also in a deficiency of contraction in the longitudinal fibres, these two together being necessary to constitute healthy peristalsis." This explanation refers the lesion to two causes, an active and a passive—one thrusting the bowels forwards into the next segment, and the other permitting this segment to receive it. The liability to this displacement seems to increase with the proximity of the small intestine to the ileo-cæcal valve, and this increase seems referable, like the effect of diarrhœa, to some general peculiarity in the muscular action of the end of the ileum, which may equally affect both the above conditions of invagination. But the remarkable frequency of the ileo-cæcal invagination seems to depend on three circumstances—(1) an irregular activity of the ileum, (2) a resistance by the valve itself, and (3) a laxity of the cæcum, probably congenital. The first tends to invaginate the end of the ileum; the second prevents the bowel from being forced through the valve; the third allows the pliable cæcum to form the middle and outer sheaths. To these predisposing causes in infancy we may very properly add two—viz., the less firm adhesion of the ileum to the iliac fossa, and the undeveloped condition of its muscular wall.

The observations of Gorham on this point are much to the same effect. He says:—"It is necessary to the production of an intussusception that there should be either (1) a contraction of the part to be intussuscepted, (2) or a dilatation of that part which is to be the outer fold, or (3) a natural and abrupt irregularity of calibre in some part of the intestinal tube. The first may be produced by spasm, the second by flatus, while the third is always present at the termination of the ileum in the cæcum. The invagination having once begun, its increase and persistence are probably due to the active peristaltic action of the outer fold, aided by the spasmodic contractions of the diaphragm and abdominal muscles,

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causing the powerful tenesmus so frequently observed. Opinions are divided as to whether enteritis holds the relation of cause or effect to intussusception."

The history of many of the recorded cases justifies the view expressed by Rilliet and Barthez, who maintain that it sometimes plays one part and sometimes the other—because the accident sometimes happens to those in perfect health, and in others its occurrence is preceded for some days by symptoms of intestinal irritation or inflammation.

The symptoms vary somewhat in different cases, some that are of much diagnostic value in young subjects being badly marked or altogether absent in adults. They may be briefly summarised as follows:—Vomiting, constipation, bloody discharge from anus, abdominal pain, tenesmus, and protrusion of intestine—the development of an abdominal tumour and tympany. Vomiting is a very constant symptom, sets in early, and rarely ceases till the invagination ceases, or till the approach of death. In infants it is rarely stercoraceous, though it is commonly so in adults. Constipation is nearly always complete, and the bloody discharge, especially in children under two years of age, is one of the most reliable diagnostic signs. Mucus is generally mixed with the blood, though the latter may be voided in such quantity as to constitute a true intestinal hæmorrhage, as in one of Mr. Gorham's cases, where an infant, aged three and a half months, passed, in a few hours, more than a teacupful of pure blood.

Abdominal pain is amongst the commonest and earliest symptoms at all ages; at first paroxysmal, and attended with contortions of limb and trunk; after a few days it becomes continuous; and, in addition, we have soreness on pressure, due to the local enteritis and peritonitis. Tenesmus is seldom absent, and adds much to the suffering; it is mostly subsequent to the abdominal pain, and may cease, like the vomiting, some time before death.

In only a small proportion of the recorded cases has an abdominal tumour been detected; when present, the left iliac fossa is its usual site, varying in size from a walnut to a large goose-egg, and giving the sensation of a solid, doughy, compressible mass.

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In adults a depression of the abdomen at a point corresponding to the displaced intestine and a fulness at the corresponding point on the opposite side, furnishes a valuable diagnostic sign; but in children this is seldom present to help us. In this class of patients, too, there is another sign of very variable frequency in occurrence—viz., the protrusion of the invaginated bowel from the anus, or the possibility of feeling it by an examination made per anum. Apart from these positively diagnostic signs, there is little else characteristic of the disease. The tongue is normal till inflammatory action sets in, when it may become dry and brown; the appetite is impaired or absent, and thirst generally moderate. Rilliet and Barthez lay stress on the diagnostic value of this last symptom, as also on the fact that emaciation is usually not so marked as in other acute diseases of equal duration and severity.

The duration of the case depends largely on the age of the patient as well as on the amount of invagination. In early infancy, should no unfavourable turn take place by the natural efforts or by treatment, a fatal issue generally ensues in five days, or, in patients a little older, seven or eight days; but when elimination of the invaginated bowel is to take place, the case is more protracted, and complete recovery may be postponed to the third week.

The modes of termination, even in favourable cases, are various—subsidence of the intussusception, either spontaneously or as the result of treatment; restoration of the calibre of the bowel by sloughing of the invaginated portion, and union and cicatrisation of the edges; agglutination of the layers of the invaginated portion, with subsequent thinning and atrophy, thus rendering the bowel pervious, though the intussusception remains. When death happens early, it is commonly due to cerebral congestion or an attack of convulsions; in most cases it occurs later, and is preceded by a stage of collapse.

We may briefly notice some other diseases from which that under consideration is at times distinguished with difficulty. A fæcal accumulation in cæcum or sigmoid flexure may be marked by constipation, occasional vomiting, paroxysmal pain, and tenesmus, so frequent and strong as to cause protrusion of the bowel. In

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addition, a well-defined tumour is present in either iliac fossa. When these conditions, or many of them, are present, a decided opinion may be impossible till we have the additional aid to diagnosis furnished by the effect of treatment. It will, however, help us to remember that fæcal accumulation is generally preceded for some days by intestinal disturbance; the vomiting is seldom so severe or constant as in intussusception; the tumour is painless, of a peculiar doughy consistence, and bloody discharges are very rare. In typhlitis or perityphlitis, pyrexia and signs of local peritonitis appear early in the case; the patient's position is characteristic, with the thighs flexed on the pelvis; an exquisitely tender tumour occupies the *right* iliac fossa; vomiting and constipation are not so marked; and, except in those cases which are preceded by dysenteric symptoms, bloody discharges are wanting.

Cholera infantum is marked by vomiting, abdominal pain, tenesmus, and occasionally protrusion of intestine from anus, but the associated signs and symptoms furnish ample ground for satisfactory diagnosis. The stools in dysentery resemble those in intussusception, but in other respects the points of difference between the two diseases are obvious.

Other forms of internal strangulation, due to mechanical causes, may produce symptoms so identical with those of intussusception that a differential diagnosis becomes impossible, but we are to some extent guided by the presence of an abdominal tumour, bloody stools, and the protrusion of the constricted bowel.

The line of treatment may be considered in a threefold aspect—*medical*, *mechanical*, and *surgical*. To relieve the engorgement at the point of constriction it has been advised to apply one or two leeches to the right iliac fossa, or over the seat of the tumour, if such can be detected. Opinions are divided as to the advisability of using laxative medicine, but if the patient be seen early, while the symptoms of intussusception are as yet obscure, a dose of castor-oil, and the administration of laxative enemata, may be resorted to with safety, if not with absolute advantage. In this connexion it is very interesting to notice that the old and discarded plan of using metallic mercury to relieve intestinal obstruction has in three

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cases recently been revived with success. In each instance the amount given was about one ounce in divided doses. If at the end of forty-eight hours such measures have failed to procure an evacuation from the upper bowel, we should desist from their further use, and content ourselves with the use of means to relieve the pain, tenesmus, and nervous irritability—such as opium, warm poultices, &c. The mechanical treatment consists in the injection of fluids or air into the bowel in such quantity as to distend it, or in the introduction of a large sound, with the view of pushing up the invaginated portion of intestine. Dr. Nissens, quoted by Rilliet and Barthez, adopted this plan with success in two cases. Inflation was recommended by Hippocrates, but the successful practice of it belongs to comparatively recent times. In the *American Journal of Medical Sciences* for 1833, and in the *Boston Medical and Surgical Journal* for 1834, will be found reports of cases occurring in adults which were cured by these means. Manifestly it is only likely to be successful when tried early, before any great amount of adhesive inflammation has taken place between the sheath and the contained intestine.

The surgical mode of treatment consists in performing the operation of laparotomy, ascertaining the site of the invagination, and gently and cautiously drawing out the incarcerated portion. Many have been found to oppose this line of treatment because of the inherent danger of the operation, the difficulty of finding the exact site of the invagination, and, perhaps, of removing the invagination, even if found.

In these days of abdominal surgery the first objection is shorn of much of its importance by the rigid observance of antiseptic precautions. To aid us in localising the point of constriction it is to be remembered that a tumour can at least in some cases be felt; that in the majority of cases the left iliac fossa contains the invaginated mass; and that the distance to which enemata pass will serve in some measure as a guide. In completing the operation we shall do well to profit by the suggestion made by Mr. Hutchinson, to seek always first the lower end of the intussusception, and to effect reduction by drawing the sheath downwards, rather than by

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drawing the contents upwards. The cases relieved by operation in the hands of Mr. Howard Marsh, and also some other surgeons, illustrate another very important point, which should be carefully borne in mind when the propriety of operative interference is under consideration—viz., that even after the lapse of several days there may still be absence of any such adhesions as would interfere with complete reduction. Even when reduction has been effected we may be confronted with the difficulty of safely returning the mass of the intestines, now increased in bulk by the removal of the constriction. Here, again, we can profit by the unfortunate experience of our predecessors as to the wisdom of puncturing the intestines to give exit to contained flatus. From some unexplained cause punctures made under these circumstances are apt to remain gaping, and thus, of necessity, a fatal peritonitis ensues, as happened in two cases, one of them recorded by Mr. Hutchinson.

My first case was a well-nourished male infant, aged four and a half months. On Tuesday morning the mother administered a dose of castor-oil, which was followed by a healthy motion at 2 o'clock. Another action of the bowels took place at 6 30. Shortly afterwards he began to cry, refused the breast, and violent straining, attended with retching, ensued, and lasted all through the night. At 3 a.m. blood poured from the bowel. Wednesday morning, free sanguinolent discharge. Wednesday night, no proper sleep; retching continuous. Thursday night, injected some olive oil, and practised inflation. Friday, 8 a.m., same treatment; 12 o'clock, repeated these measures, each time with manifest relief to the straining and retching. 9 p.m., treatment continued. 12 p.m., a fæcal discharge passed, after which there was no return of the retching, and convalescence progressed steadily, though for some days there was moderate fever, attended with much thirst.

My second case was a healthy male infant, aged twelve months. On Sunday morning was in perfect health, and eat a hearty breakfast. At 10 30 a.m. passed a rather hard evacuation from the bowels; 11 30 after a bath became ill, grew pale and heavy, retching supervened, and blood and mucus passed from the bowel. Much in the same way all day—incessant vomiting and tenesmus. 10 p.m., passed catheter, and inflated bowels; some sleep was procured, but on Monday vomiting was still very troublesome.

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12 o'clock, inflation; 12 p.m., no vomiting or straining since; treatment repeated. Tuesday morning, passed a restless night, but there has been no return of vomiting or straining, and no discharge of any kind per anum; does not take nourishment so well as before; some blood and mucus escaped from the bowel when I passed the catheter; some difficulty in getting it up, but this was overcome, and it entered to the length of ten inches; more tympany to-day than yesterday. 9 p.m., looks better; no vomiting nor discharge from bowels; has had some sleep. Restless, and inclined to start and cry during the night. Wednesday, much difficulty experienced in passing tube high up; no return of bad symptoms. 8 30 p.m., some fluid fæcal matter escaped, and later in the night copious stools were passed. For some days afterwards there was much pyrexia and abdominal tenderness, but perfect recovery followed.

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PATHOLOGICAL SECTION.

INTUSSUSCEPTION OF THE SMALL INTESTINE.

By KENDAL FRANKS, M.D. Univ. Dubl.; F.R.C.S.;
Surgeon to the Adelaide Hospital, &c.

[Read in the Pathological Section, November 7, 1884.]

In the June Number of the *Dublin Journal of Medical Science*, I published two cases of intestinal obstruction, one of which was due to an intussusception involving the ileo-cæcal valve, and for the relief of which I performed abdominal section. The specimen which I now present to the Pathological Section of the Academy of Medicine illustrates an intussusception confined to the small intestine, and is of an unusual length, involving thirty inches of the gut. It is well, I think, to compare these two cases together, as the former illustrates the chronic form, lasting for weeks or months, whilst the latter is an instance of the acute form, whose duration is counted by days.

Case.—The patient from whom this specimen was obtained was a clerk, aged thirty-four, who was admitted to the Adelaide Hospital, under the care of my colleague, Dr. James Little, on the 18th of October, suffering from acute obstruction of the bowels. He had always been a healthy man, never having been laid up with any illness, though he was not altogether addicted to temperate habits. His usual allowance was, besides a glass of stout at his dinner, four to six glasses of whisky per day. On Saturday, October 11th, his diet was as follows:—For breakfast, tea, bread and butter; for lunch, a sandwich and a bottle of stout; for supper, tea, bread and butter again. This not very copious diet was supplemented with 3½ glasses of whisky. He retired to rest that night as well as usual, but at 1 o'clock on Sunday morning he

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awoke with a violent pain in the epigastric and umbilical regions. The pain was of a growing character. Feeling as if the bowels were about to be moved, he attempted to relieve himself, but nothing passed from him. The effort, however, produced vomiting, the ejected matter consisting merely of his supper of the previous night. The vomiting continued all through the night, at intervals of ten or fifteen minutes. About 4 o'clock in the morning he observed that his abdomen was enlarging. As this enlargement increased the pain gradually subsided, and was replaced by a feeling of soreness. At 7 on Sunday morning he took three antibilious pills and a draught, but these were immediately rejected. By the evening the pain had completely subsided. He continued in the same state till Monday evening, when, as he had not even passed wind, he got an enema, with, however, no result. Finding that he was making no improvement, and that the constipation was absolute, he applied at the Adelaide Hospital, and was admitted on the following Wednesday evening—that is, on the fourth day since the illness declared itself. He then presented the following symptoms:—He complained of a great feeling of rawness and soreness over the whole abdomen, with tenderness on pressure, especially when the pressure was firmly applied. The abdomen was generally enlarged, very tense and unyielding; it measured 31½ inches at the umbilicus, and 32½ two inches above this. The umbilicus was slightly protruded. Percussion showed dulness in the right hypochondriac region, extending downwards into the right iliac fossa and backwards into the right lumbar region. The rest of the abdomen gave a percussion note somewhat higher than usual. The skin over the abdomen was stretched by the swelling, and there was no evidence of a tumour in one place more than in another. The tongue was covered with a dry brown fur, the edges were red and clean. He complained of intense thirst, from which he had suffered since the previous Sunday, and there was a bad taste in the mouth. He could not appease his thirst, as every attempt to drink anything brought on vomiting. This vomiting had been constant, at short intervals of ten or fifteen minutes, ever since the attack came on, and the ejected materials were now distinctly fæcal in character; his breath, moreover, had a strong fæcal smell. The bowels were obstinately confined, neither fæces nor flatus having passed since the previous Saturday. He had no trouble with his urine.

On Thursday, the 16th October, an enema, large in quantity,

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was administered, but it brought nothing away with it. He got a hypodermic injection of morphia that night.

On Friday, the 17th, another enema of about two quarts was administered by means of an O'Beirne's long tube. This tube was with little difficulty passed to its full length. The enema failed to carry anything back with it, except a little dark-coloured blood.

On Saturday morning, Dr. Little and I saw the case together. The patient had then an anxious, haggard look, and complained of great weakness. The symptoms were unchanged, except that there seemed to be more tenderness—chiefly referred to the right side, and most intense over the eleventh and twelfth ribs. He stated that he had passed some wind during the night, and while we stood by the bedside he undoubtedly passed some more. The abdominal muscles were all in a state of tension, but there was no spasm or spasmodic pain. Visible peristalsis was quite absent. Dr. Wallace Beatty examined the urine, and found that it contained a large quantity of indican, which suggested that the obstruction was in the small rather than in the large intestine. A consultation of the staff was then held, and it was decided that operative measures should be resorted to the following morning, should further attempts to relieve the obstruction by means of enemata during the day fail. It was hoped that the passage of flatus during the previous night and in the morning might indicate that the obstruction was about to give way—a hope which was not, however, realised. Another large enema, administered with O'Beirne's long tube, brought away a good deal of dark blood with a fæcal odour, but nothing else. Accordingly, the following morning, October 19th, I opened the abdomen by an incision from the umbilicus to the pubis. A piece of deeply congested and distended small intestine appeared in the wound. Adopting Dr. Frank Rand's method of determining in which direction I should follow the intestine in order to reach the obstruction, a method which had proved so serviceable to me on a former occasion, I quickly came upon an intussusception extending from the right iliac fossa upwards to the liver, and measuring ten inches in length. The small intestine only was involved. I drew this outside the abdominal cavity, protecting the wound and any intestines which endeavoured to escape with towels wrung out in hot weak solution of carbolic acid. The intestine involved was of a dark purplish colour, almost green in some places, and greatly distended. The upper end of it was very hard, and that portion which was involuted was very tightly constricted at its neck. I

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first endeavoured to undo the invagination by gentle traction with my left hand on the upper end, whilst I grasped the lower end with my right hand and gently kneaded it, using a sort of circular pressure. This proceeding had not the slightest effect, and I, therefore, determined to relieve the extreme tension of the parts by puncturing the gut and drawing off the contained fluid with an aspirator. This fluid proved to be blood-stained serum. The point of entrance of the aspirating needle, after the withdrawal of the latter, was secured with a catgut suture. The kneading and traction processes were resorted to again, but for a long time were futile. These had to be of the gentlest kind, as the peritoneal covering of the intestine showed a disposition to give way readily. This happened in two or three places, and the rents were immediately sewn up with fine catgut. The intestine itself did not give way anywhere. Gradually it was found that the involution was beginning to yield, but when it had become undone in about half its length it resisted further efforts for a considerable time longer. Finally, however, it was completely disentangled, and it was carefully replaced in the abdomen. The abdominal wound was then closed carefully, and the dressings were applied. The operation had lasted for over two hours, and when the patient was returned to bed but little hope of recovery was entertained. He did not rally after the operation, but, sinking gradually, he died the same night. A *post mortem* examination was made by Dr. Wallace Beatty; the whole length of the intestine involved, including both the invaginated portion and the sheath which contained it, measured thirty inches. The distal end was situated in the ileum at a distance of thirty-three inches from the ileo-cæcal valve, consequently the upper portion was sixty-three inches distant from the valve. The colour of the affected portion was of a dusky-red colour, purplish in many places, and in a few places seemed to be threatening gangrene. There was no extravasation into the peritoneal cavity. There was no peritonitis, except at the distal end of the invaginated portion, where there was a very small quantity of lymph exuded. There were no adhesions between the several layers of the involved portions of the intestine, and no attempt at a natural cure seemed to have begun. The ileum above the seat of invagination was greatly distended, containing fluid fæces similar to what the patient has vomited. The intestines below the intussusception were empty and somewhat contracted. The liver, spleen, and kidneys were healthy.

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This case illustrates that form of intussusception which we may term *acute*. The onset of the attack was sudden, accompanied by great pain, and had occurred only three days previously to his admission to hospital. It differs thus markedly from the case reported by me in the *Dublin Journal of Medical Science* for June, 1884. In the latter case the first evidence of the volvulus appeared six weeks before he applied at the hospital. The onset was, apparently, very gradual, as the first thing which attracted his attention was a tumour in the abdomen, accompanied by pain which at first was not intense, and was not constant. In comparing the symptoms which presented themselves in this chronic case, we find a great divergence between them and those which accompanied the acute intussusception. In the acute case the pain for the first few hours was agonising, but then it gradually subsided until, in a short time, it was replaced altogether by a "feeling of soreness." This was constant, and was not marked by any exacerbations. On the contrary, in the chronic case the pain came on gradually, increased in severity, and was paroxysmal; the paroxysms were separated by short intervals of a few minutes. Again, in the acute case, there was constant vomiting, fæcal in character, and, during one day, hiccough. In the chronic case, he stated that previously to admission he had frequent vomiting, but during the four days spent in hospital previously to operation there was *no* vomiting and no hiccough. In this latter case, also, there was strongly-marked visible peristalsis, which was quite absent in the acute case. In the chronic case, again, there was no blood ever passed per anum, whilst on two occasions—the day of operation and the day before—in the acute case, blood came away with the enemata which had been administered.

The rapid subsidence of the pain in the case at present recorded, and the total absence of paroxysmal returns of it, are very unusual in cases of intussusception. It is in the ileo-colic forms, and in those involving the small intestine, that the pain is most severe, being due to excessive and irregular peristalsis. It is, therefore, difficult to explain why, in the present case, it should be chiefly conspicuous by its absence.

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I have brought this case forward before the Pathological Section of the Academy on account of the many interesting and instructive points it illustrates, both clinically and pathologically.

In conclusion, I would, however, refer to another lesson it teaches. When such a case presents itself, the difficulties in diagnosis are so great, and so much is hoped often from the milder measures of inflation and enemata, that we hesitate to resort to the more heroic measures offered by surgery; and so it is that valuable time is lost, and surgical interference fails to triumph, not because it is not able to do so, were it accorded fair play, but because it is held over till all chance of success has passed away, and we can then only lament that it came "too late." Such was the history of ovariotomy, and such will be the history of intussusception, until we learn the lessons taught by such cases as the present; and, I believe, when we do so learn, and when we operate early, we shall find that success in such cases will not compare unfavourably with what has already been achieved in the other departments of abdominal surgery.

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REMOVAL OF LARGE SARCOMATOUS TUMOUR OCCUPYING THE HYPOGASTRIC AND PART OF THE EPIGASTRIC REGIONS OF THE ABDOMINAL PARIETES.

By WILLIAM. I. WHEELER, M.D., M.Ch.;

Fellow and Past-President Royal College of Surgeons; Surgeon to the City of Dublin Hospital, &c.

[Read in the Pathological Section, November 7, 1884.]

The patient who was the subject of this affection was a fine, strong-looking girl of nineteen years of age, living in the country all her life, and enjoying that healthy existence which is usual under such circumstances. She was of medium height and well developed; but her face had a peculiar ashen colour, which was in strange contrast to her otherwise healthy appearance.

On admission to the hospital one was immediately struck by her outline, and at first she gave the idea of one *enceinte*, except that at the point at which her dress was most protruded there was a moist stain that had saturated through all her clothes during her passage up in the train, and when she walked she threw her shoulders slightly backwards, in order to support the enormous tumour that hung pendant from her abdominal walls.

On examination, the tumour presented the following appearances: Rising gradually from the umbilicus it attained a prominence of about seven inches in front of the normal abdominal walls, and was surmounted by a large ulcerated patch, which exuded a thin, watery fluid that rapidly became sanguineous under the least handling, and completely revealed the nature of the growth. By a rapid descent it reached the pubis, which it might be said to have overhung, and in its forward growth had not alone caused the mons veneris to be its lower surface, but had drawn on the labia majora

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to such an extent that the vulva remained widely open, although the legs were approximated. Laterally its extent was more limited than one would suspect, considering its height; but its anatomical relations, as afterwards revealed, fully explained the cause. Suffice it to say that the horizontal inguinal glands and superficial epigastric vessels occupied a position high on its lateral aspect. The glands just mentioned were mostly normal in size, but one or two (those connected with the labia and mons veneris) were as large as walnuts. The surface of the tumour was covered by a coarse form of leathery skin, and was below overgrown with the hairs of the mons veneris. To the hand, the tumour felt freely movable from side to side, and its peripheral outline could be indistinctly discovered all round, except at the pubis, where the structures appeared to be very tensely stretched. Its weight at once struck the observer, and the inability to cause it to slide on the abdominal walls, although the skin round its base was quite freely moveable on the tumour itself. In point of consistence it was firm to the touch, unyielding, except what the abdominal walls allowed, and no vibration could be discovered that would give the idea of large vessels passing through its structure.

She stated that it had grown in thirteen months, having begun in July, 1883; that from modest motives she tried to hide it, till at length its proportions excluded any chance of further concealment; that pain was never very troublesome, and that as regards her health, menstrual and otherwise, she had never been a day "wrong."

By a careful examination I determined that the uterus and appendages in no way were implicated in the growth, although the bladder, from its close proximity, could not for certain be considered free, especially on its upper and anterior surface; but as there was no history of frequent micturition, or of bladder mischief, I concluded that although the latter viscus might be coterminous, it was not continuous with the tumour.

Pulse, temperature, appetite, sleep, excretions, and secretions in no way deviated from normal.

Having carefully considered the expediency of operation as a *dernier résort* for her life, I duly proceeded to operate in the

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following manner:—She was placed under the influence of ether, and having made two semilunar incisions, meeting above and below, which traversed as high a point on the tumour as the healthy skin permitted, I rapidly dissected back the flaps so formed till I reached the periphery of the base of the tumour. While doing so, the epigastric (superficial) vessels sprang, and showed, by the copious hæmorrhage that followed their division, what an enormous size they had attained in endeavouring to supply so large a mass, and further confirming my opinion that it was essentially a tumour of the abdominal walls. The vessels were rapidly seized by clip forceps. I now proceeded to separate the base of the tumour on each side, and found doing so fraught with danger, as the recti-abdominales were spread out and absorbed over its surface, and were consequently lost as a guide to the depth I might proceed to, without opening the peritoneal cavity. Having freely detached them, I next proceeded to liberate the growth above, leaving the more fixed attachments below as a means of steadying the unwieldy mass. I found the tumour was entirely in front of the peritoneal membrane, and I now proceeded cautiously to dissect off the tumour from the sub-peritoneal tissue. In doing so I soon perceived the internal epigastric vessels passing towards the umbilicus, and it required an amount of care that can readily be imagined when it is remembered that the abdominal walls were moving rapidly with each respiration. Arrived at the pubis, and allowing the tumour to rest between her legs, I found the pedicle to consist of a somewhat firmer band of structure than the rest, about the size of two fingers and one-half inch in length. It was firmly attached to the symphysis pubis and the superior inter-pubic ligament. Its entire (peduncular) attachment could have been covered by a postage-stamp, and having divided that, I now proceeded to remove the gland that closes the femoral opening at the septum crurale of Cloquet, and which was about the size of a goose-egg. In doing so it became necessary to divide the obturator artery, which, in this case, took its origin from the external iliac artery. The surface of sub-peritoneal tissue exposed (for the fascia transversalis appeared to form the posterior

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lining of the tumour) was about five inches by four; and having satisfied myself that no bleeding points were left, I brought the wound together by harelip pins and figure of 8 sutures. Large pads of lint, with straps of plaister, covered with flannel rollers, were applied, and the dressing was complete.

During the operation she became extremely weak, so much so that on one occasion I found it necessary to suspend the operation till I had raised her legs and applied Esmarch's bandage from the toes upwards. She recovered wonderfully, but was still too weak to remove her from the table. I had warm jars placed round her, and gave her constant mouthfuls of champagne, with a little digitalis. She rapidly recovered consciousness, and was able to reply to questions as to how she felt.

In two hours she was sufficiently recovered to have her removed to bed, where she rallied well, although she did not sleep. One hour and a half after being placed in bed she suddenly became restless, made several attempts to sit up in bed, and in about half an hour expired, although she received hypodermics of ether, &c., having lived four hours after the operation.

The tumour was of the spindle-celled variety of sarcoma, and weighed 10 pounds 6 ounces. It appeared to be traversed by large blood spaces. It was the first of the sort springing from the pubis that had been found in females.

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INCOMPETENCE AND EXTENSIVE DISEASE OF AORTIC VALVES.

By SURGEON-MAJOR HAMILTON, M.D.

[Read in the Pathological Section, December 5, 1884.]

G. B., aged thirty, of fine physique, was admitted to Portobello Station Hospital on the 1st of October, 1884. The man belonged to a battery of the R.H.A., but for some time was employed on the provost police, on which duty he had only to walk about, and thus escaped the heavier work of an R.H.A. man. I am informed that he would never run, as he knew for some time that his heart was affected.

There was a distinct history of constitutional syphilis. He came to hospital complaining of palpitation, and on examination organic disease of an advanced character was recognised. There was extensive precordial dulness, a well-marked systolic bruit at apex, and over base a loud systolic and also a regurgitant bruit. The man was proposed and passed for invaliding, and for nearly a month remained in much the same condition.

On the 25th October, nearly four weeks after admission, his legs began to swell. His urine was tested, and found free from albumen. On the 26th he was worse, the dropsy was rapidly advancing, and fluid was diagnosticated in the abdominal cavity. On the morning of the 27th he was much worse; face livid, breathing laboured, pulse small and weak; dulness over precordial region increased; dropsy had extended considerably; heart's action tumultuous, and sound over base one loud murmur, it being impossible to differentiate between the systolic and diastolic bruits. That night the man sat up contrary to orders, and died almost immediately.

Post mortem examination thirty hours after death.—Body well nourished; rigor mortis well established. Head not examined.

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Chest—fluid in cellular tissue; both pleuræ contained fluid; pericardium distended with fluid. Lungs healthy. Heart enormously enlarged; weighed one pound twelve ounces; removed for subsequent examination. Abdomen—much fluid in cavity; all viscera normal.

"*Description of Heart with Incompetent Aortic Valves.* By P. S. Abraham, F.R.C.S.

"The cavities of the heart on both sides are dilated, and the walls are everywhere hypertrophied, but otherwise apparently healthy. On the right side the auriculo-ventricular valves are normal, and the only lesions observable in the semilunars of the pulmonary artery are a slight thinning in one of the cusps, and a reddish thickening in the corner of another—viz., the left anterior cusp. The inner surface of the pulmonary artery is of a remarkably deep purple colour. On the left side one of the mitral cusps presents some thickening at its edge, but, like the valves on the right side, there is no evidence of incompetency or of stenosis. In the left ventricle, near the aortic opening, there are some spots of commencing atheroma. As on the other side, both the cavities contained *post mortem* clots.

"On pouring water into the aorta its ventricular orifice was found to be widely patent, and, before splitting up the vessel, the finger could distinguish a very abnormal condition of affairs. The valve seemed thick, rough, and frayed; a hole of considerable size could be determined in one place, and just above and behind this a hard nodule, and a cavity below it, could be distinctly felt.

"The vessel was then laid open from the ventricle, and the appearances within it are as follows:—Bulging patches of atheroma—not advanced to calcification—are extensively deposited in the ascending arch, and a few smaller patches lower down opposite valve, and on the bottom of the sinuses of Valsalva.

"Between the openings of the coronary arteries, and at their level—*i.e.*, nearly at the point where the anterior segment and the left posterior segment of the valve should meet, is situated an ovoid mass of bony consistence, measuring 9 mm. in its longest,

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and 7 mm. in its shortest, diameter. The projecting surface of this mass is rough, and one or two sharp spicules can be felt, but a great part of its surface is covered by 'vegetations.'

"Just beneath this nodule a cavity, nearly 2 cm. in length and 1 cm. in depth, opens below the level of the bottom of the sinus, behind the endocardium, and into the muscular substance of the heart—*i.e.*, a dissecting aneurysm has been formed.

"The anterior and left posterior cusps of the valve have become confluent, and at the region of the junction—*i.e.*, opposite to the bony nodule, a wide opening, 1·5 cm. in diameter, exists, with a fringed border. The remaining parts of the valve are thickened, and an ulcerated spot is to be seen at one point of the ventricular surface."

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OSTEOMA OF THE SUPERIOR MAXILLA.

By ARTHUR W. W. BAKER, M.B., Ch.M., Dublin;
Surgeon to the Dental Hospital of Ireland.

[Read in the Pathological Section, December 5, 1884.]

Osteomata, or exostoses of the alveolar process, receive but slight attention from most writers on dental surgery; and such growths are chiefly noted by them as appearing on the lingual surface of the lower jaw, where they are far from uncommon. Mr. Tomes, however, describes a case in which a bony ridge projected like a shelf from the labial surface of the upper jaw, the only inconvenience to the patient being the retention of food on this shelf. A somewhat similar case occurred in my own practice, where, in a lady, the labial surface of the upper jaw was studded with little nodules of bone, which ran in a continuous line, about the level of the roots of the teeth; these little growths used to ulcerate through the gum from time to time, exfoliating a small scale of bone. This patient, when she consulted me some two or three years ago, was in a state of great fright, thinking that she was the victim of a malignant disease. I, however, assured her that there was not the least cause for alarm, and in this opinion I was supported by Mr. Stack, Mr. West, and my father, who saw the case in consultation with me. Our prognosis has been fully verified, for the lady came to consult me recently, and I find she has had no further trouble.

In Wedl's Pathology of the Teeth he states that he has met with osteomata appearing as if they had been dropped upon the alveolar process of the upper jaw,* only upon the facial surface, and corresponding to the localities of the canines, bicuspids, and molars. In the case which forms the subject of this communication the

* Similar growths occupying the centre of the hard palate are familiar to most dental surgeons. I have a cast of an excellent example in my possession.

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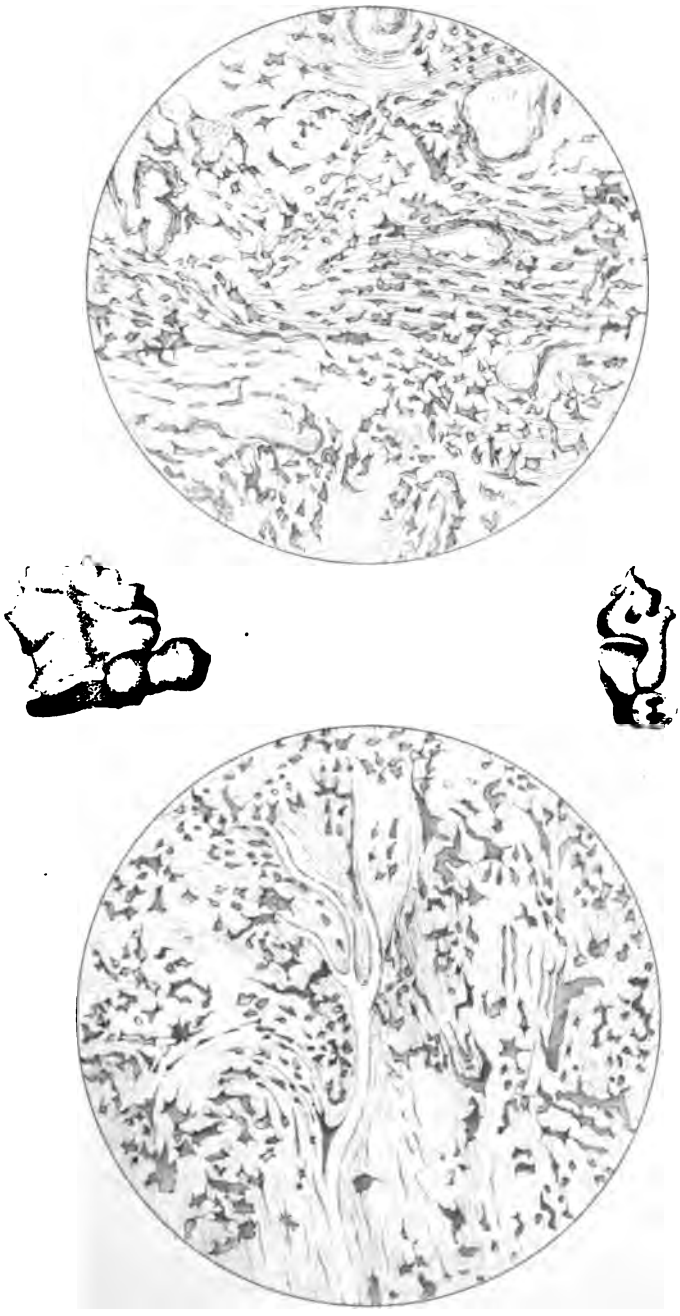

Mr A W Baker on Osteoma of Superior Maxilla

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osteoma sprang from between the palatine and facial alveolar borders—in fact, occupied, to a certain extent, the position of the first left upper molar tooth.

About the beginning of December last year, Miss ——, aged forty-five, was sent to me by my colleague, Mr. Story, under whose care she had been for an abscess in the right ear. It was, however, on account of a tendency to deafness in the left ear that Mr. Story wished her to see me, he regarding the irritation of the fifth nerve by some roots in the upper jaw on the left side as a probable cause of the deafness.

On examining the patient's mouth, I found in the left half of the upper maxilla the roots of the third and second molars; the first molar was absent, and in the position it should have occupied the gum presented an unusually rounded appearance, with a small opening, through which exuded a few drops of cystic-looking fluid. The opening was unlike that which usually leads to a root. The second bicuspid was standing and in good condition; the first bicuspid and canine were wanting. The pulps in the central and lateral incisors were dead, and on the labial side of the gum, about the canine eminence, there was an opening similar to that which I have described as existing over the socket of the first molar, and discharging a similar fluid.

From the history the patient gave me, I found that the first molar and first bicuspid had been extracted, but I could not find out whether the permanent canine had ever been erupted—at all events, I could find no history of its ever having been extracted. The cyst towards the front of the mouth had been noticed for the last eight or ten months, but, beyond the discharge, caused no inconvenience.

I decided, in order to carry out Mr. Story's views, to remove all the roots on the left side of the jaw, and to endeavour to effect a closure of the cyst.

I removed the roots of the third and second molars, and proceeded to grasp with the forceps what I thought would prove to be a root of the first molar, but failed, and, instead, came upon something unusually dense. I then divided the gum, which was freely movable and not attached to the growth, and passed in a probe to examine it.

From the enamel-like and nodulated surface, rendered very slippery and indistinct by blood, and more especially from the sulcus

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on the buccal surface, I thought I had come upon the crown of a buried or unerupted tooth, or, failing that, an odontome springing from the root of the second bicuspid.

Being unable to move this growth by any ordinary force, I determined to remove the second bicuspid, thinking that, if it were an odontome, perhaps the removal of the tooth would bring the growth along with it. (Like the case described in Mr. Heath's work on Injuries and Diseases of the Jaws, third edition, p. 224.)

I removed the tooth, but the growth remained, and I afterwards removed or rather fractured it off from the alveolus with the forceps, being quite unaware at the time of its real nature.

At a subsequent visit I made an examination of the cyst, which I described as presenting a small opening on the labial surface of the gum, near the canine eminence. This opening was very like the opening that had existed over the little tumour, and through it, as I have said, exuded a similar fluid. On passing a probe in I was unable to touch anything but soft walls all round. The cavity extended from the middle line back to the socket of the second bicuspid, and I found that a considerable portion of the hard palate was absorbed. As far as I could determine, there was no connection between this cavity and the little tumour I had removed.

On different occasions my friends, Dr. Stack and Mr. West, happened to call while the patient was with me, and my patient and I had the advantage of their opinion. They agreed with me in supposing that the cyst was most likely due to the retarded eruption of the canine tooth, although we could not detect it in any part of the jaw. We also satisfied ourselves that this cavity was not connected with the antrum. The treatment I had adopted also met with their approval—viz., syringing out the cavity with carbolic lotion containing about two per cent. of the acid, and plugging the cavity with cotton wool. Subsequently I removed the left upper central and lateral incisors, as their roots could be plainly felt in the cavity, and as they possibly might be preventing the closure of the cyst. The few days succeeding the extraction the patient suffered a good deal of pain, and had some epistaxis from the left nostril. I may add that for an unusual length of time the sockets showed no signs of closure.

When I thought of making this communication I wrote to my patient, requesting her to call on me. Subsequently I received a note from her, stating that she was out of town, and hoped to call

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on me on her return, adding that the discharge from her gum still continued, and that there was some soreness in her nose on that side.

The extreme difficulty of forming a correct diagnosis in this case as to the connection, if any, between the extensive cyst in the front of the jaw and the little osteoma which I removed; and further, when dealing with the tumour, as to whether it was an osteoma, an odontome, or a buried tooth, may be seen by the fact that all of these pathological conditions have some points in common. The absence of one or more teeth from their proper place is a sign which, according to Mr. Tomes, has characterised all the cases of odontomes on record. I need hardly say how this sign would apply to a buried tooth; and knowing the vagrant disposition of the canine, which in this case was the missing tooth, it was hardly assuming too much to suppose that it might have undergone warty development, a condition described by Salter, and migrated to the position of the first molar; and, as I have already stated, the hard, slippery, nodulated surface, rendered indistinct by blood, could not readily be distinguished from the crown of a tooth.

Cysts, we know, may be developed in connection with either odontomes or buried teeth. Mr. Tomes supposes in the latter cases that when the enamel is completed, its outer surface becomes perfectly detached from the investing soft tissue, and that a small amount of transparent fluid not uncommonly collects in the interval so formed. This fact would fully explain how cystic tumours in connection with buried teeth arise. The little cyst which I noticed in connection with this osteoma may be explained by the fact that the small amount of irritation set up by the slow growth of the tumour was not sufficient to produce suppuration, as when we find little cysts in connection with the roots of teeth where a more acute morbid process would have occasioned alveolar abscess (Tomes). The appearance of this little tumour, both to the naked eye and under the microscope, accords with the description given by Wedl and Heath of osteomata. It has a broad base, polished ivory-like outer surface, and is traversed by vascular grooves, both on its surface and internally. On section the lacunæ are seen irregularly disposed

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round the vascular canals which penetrate the tumour for some distance; nearer the periphery, however, some lacunæ may be seen arranged in parallel rows.

Wedl is of opinion that these growths when they appear on the facial and lingual surfaces of the jaws occur in connection with powerful jaws and strong sets of teeth, or with closely-set or overlapping teeth, and that the act of mastication is conducted with such force as to produce displacement of the lower teeth towards the tongue, and the upper teeth towards the cheek.

The removal of neighbouring diseased roots frequently causes these growths to disappear. Mr. Stokes informed me of a case which was under his care at the Richmond Hospital, where a largish growth of this description disappeared in about six weeks after the removal of some roots near it. Should such a case come under my care again this is the treatment I would adopt before attempting the removal of the tumour by other means.

In conclusion, from the absence of the canine tooth, the discharge near the canine eminence, and the irritation about the left nostril, I fear this patient is likely to have some further trouble, probably in connection with the left antrum.

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A CASE OF CALCULUS OF THE TONSIL.

By F. ALCOCK NIXON, F.R.C.S.;
Surgeon to Mercer's Hospital;
Lecturer on Surgery in the Ledwich School.

[Read in the Pathological Section, December 5, 1884.]

A. B., aged twenty-five, well known in the literary and musical worlds, came to me complaining of his throat, which, he said, he thought was inflamed, and of some difficulty in swallowing.

On examination I found congestion of the soft palate and fauces generally, and enlargement of both tonsils, but more especially the left one, on the surface of which I observed a yellow spot, which, when struck with a probe, gave a metallic ring. On further examination I detected a calculus, which, after some difficulty, I removed with my finger and a lithotomy scoop. There was very free hæmorrhage, which was, however, readily arrested.

I exhibited the calculus on the day of its removal at the Academy of Medicine, when it was referred to a committee for examination. I therefore was not able to weigh it, or to ascertain its composition by analysis.

I thought the case worth bringing before the notice of the profession, as I have been unable to find any record of a similar one.

It seems strange that this concretion, as large as the first joint of my thumb, should have remained embedded in the tonsil without causing any inconvenience whatever, until the attack of general congestion, the result of an ordinary cold, supervened and produced difficulty of swallowing. Yet such was the case, for this gentleman was able to sing in public two days before I saw him. He attributed the condition of his left tonsil to the fact of his having a discharge from his left ear, in which he was slightly deaf, owing to a blow which he received when a schoolboy.

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MAMMARY TUMOUR IN A GIRL OF TWELVE.

By JOHN K. BARTON, F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Pathological Section, December 5, 1884.]

The patient, a very fine bright-looking girl of twelve years of age, but presenting all the appearance of being fourteen or fifteen, noticed a swelling in the left breast towards the end of September last. When first noticed, this swelling was about the size of a pigeon's egg, and quite painless when pressed, but the breast felt tender. The swelling increased so much in six weeks that her parents became alarmed, and determined to bring her up to town for advice from Kilkenny, where she had been under Dr. Lyster's care. She was admitted to the Adelaide Hospital under my care upon the 22nd of November, just two months from the first appearance of the disease, when the breast presented upon examination the following signs:—It formed a globular projecting swelling, being, as judged by the eye, four times as large as the other gland; large blue veins ramified throughout the delicate skin which covered it—in this respect also contrasting with the healthy breast. When examined with the hand, the swelling, which to the eye seemed uniform and globular, was felt to be lobulated, freely movable upon the muscle behind and beneath the skin, being quite unattached to either. The tumour could not be separated from the gland; it seemed to be a tumour of the gland; it had an elastic, but not a fluctuating feel; handling gave no pain; the nipple was not retracted; there were no glands enlarged in the axilla or beneath the clavicle.

The history given by the mother of the girl was to the following effect:—Annie is one of a large family; both parents are alive and

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well; one brother is now in hospital suffering from severe disease of the spine; a grandmother died of cancer of the breast, at the age of seventy, which had recurred after operation.

The family had been in very comfortable and highly respectable circumstances, the father, besides farming some land himself, acting as agent for landlords in his neighbourhood; but times were changed. He lost his place, and having nothing but his farm, found it very hard to support his numerous family. In order to present a respectable appearance it became necessary to reduce the expenses of living, and the children had been for more than two years fed upon insufficient and coarse food. No hurt or injury had been inflicted, and certainly the high colour, bright eyes, and healthy laughing face of the girl did not look like starvation; but I am satisfied from the account given to me that she had but rarely tasted meat during the past two years.

The diagnosis was arrived at after a careful elimination of tumours which it could *not* be; and, finally, I ventured to say this must be either an adenoma or a sarcoma; in my opinion it is the latter.

The treatment by complete removal followed from this diagnosis. The operation was performed upon December 2. A long thin ellipse of skin, including the nipple, was cut, and then the whole gland and tumour were rapidly dissected out; numerous veins bled; a few small arteries required ligature; the wound was washed with chloride of lime solution; a small drainage-tube inserted at each angle; the edges came well together. All was done antiseptically, the gauze being prepared with a 1 per cent. sublimate solution in serum of horse's blood. The process of healing went on uninterruptedly and without any suppuration whatsoever, 98·8° being the highest temperature noted. The patient was sitting up in a week, and quite well in a fortnight.

I submitted the tumour at once to Mr. Abraham's examination, and the following is his report:—

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"*Report by Mr. P. S. Abraham, F.R.C.S., on the Structure of a Mammary Tumour, excised from a girl, aged twelve years, by Mr. J. K. Barton.*

"Macroscopically, the consistence of the tumour is soft, pinkish white, homogeneous, and elastic; the cut surface bulges out, and the whole growth can be easily peeled out from the surrounding tissue—*i.e.*, it may be said to be encapsulated. The stained microscopic sections exhibit to the naked eye a fine mottling, and numerous small irregular spaces which have been cut across. Under a low power these spaces are seen to be enlarged glandular alveoli, lined with epithelium, and separated by thick septa. The latter, which make up the greater part of the whole substance, appear, when more highly magnified, to be made up largely of spindle-shaped embryonic cells, as in a sarcoma, but in many parts this growth has become developed into a succulent fibrous tissue, full of elongated and other shaped cells, irregular protoplasmic bands, and with small spaces between all these elements. The smaller acini are often nearly filled with proliferating epithelium, but the larger ones have several layers of cells circumferentially disposed around the vacant lumen. The more external cells are large, with the protoplasm but little stained, and are regularly placed; the inner ones are smaller, more rounded, and they have strongly taken up the stain. The basement membrane of the glandular epithelium is not always distinct, and sometimes it is difficult to differentiate these cells from the neighbouring ones of the surrounding tissue. The growth may probably be considered to be an adenoma, with cellular hyperplasia of the interalveolar tissue; the only suspicious feature in the neoplasm being the large amount and embryonic character of the latter."

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CONGENITAL DISLOCATION OF THE HIP.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; Professor of Surgery, Trinity College.

[Read in the Pathological Section, January 16, 1885.]

In fulfilment of the promise which I made in the course of my address in opening the Session of the Surgical Section of the Academy this winter, I now exhibit this specimen of congenital dislocation of both hips. I am the more anxious to do so because custom prevented any discussion arising out of my observations in the Surgical Section, and so any criticism of my views regarding the pathology of this dislocation was not possible. These details are the proper subject of this meeting, and I am quite ready to discuss them with all freedom.

The child from whose body I obtained this specimen was six years old, a female. She was transferred to my care in Sir P. Dun's Hospital by my colleague, Dr. Ball, a day or two after her admission to hospital. Her parents brought her for treatment, as the peculiarity of her gait of movement, which had existed ever since her earliest attempts at walking, was becoming more developed. She could stand firm and erect, but with an undue projection backwards of the buttock. Her attempts at walking were most defective and waddling, but she could run with rapidity and ease. Her legs were widely separated at the knees, and the limbs were neither inverted nor everted. The buttocks, though prominent from the attitude of the pelvis, were small in consequence of the atrophy of the gluteal muscles present. When she lay on the back with her limbs extended the knees could not be made to touch, the trochanters were placed above their natural level, and were slightly prominent, while their position could not be altered by traction in the axis of the limbs. The casts taken from the dead

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body exhibit these features, but somewhat exaggerate them, for the child was emaciated by illness, and so the limbs appear unnaturally wasted. Very soon after the child came under my care, and before I had time to exhaust the examination of her deformity, she took scarlatina. She suffered very severely, and after the subsidence of the eruption fever persisted. After desquamation had occurred the phenomena of tubercular meningitis followed, and so she died. During her illness she was constantly visited by the monthly nurse who had attended her mother during her confinement, and who lived in her house. This woman had been educated under my late colleague, Sir E. B. Sinclair, and had large experience as a midwife. She assures me the birth of the child was normal—a head presentation, presenting no complication.

The dissection exposes the two hip-joints, the great gluteal muscles being reflected. The fibres of these are in great measure degenerated, replaced by fibrous tissue and fat. The deeper muscles are also greatly wasted. The general contour of the pelvis, exposed by the dissection of the muscles, is the same as in the specimens preserved in our museum, which were dissected by Harrison, and, in one instance, by myself. The alæ ilii appear more erect than normal, and the crests are slightly approximated. The anterior spines are made to project unnaturally by the development of the deep groove beneath the inferior, which lodges the psoas and iliac tendon. The tuberosities of the ischia are, as in the other specimens, unduly prominent externally. The position of the joint containing the femoral head on each side is above and behind the site of the acetabulum, but there does not exist any projection or tumour behind the trochanters, as is to be seen in traumatic or pathological dislocations backwards. On moving the joints the heads of the femora can be made out, placed in front of and internal to the trochanters. The capsular ligaments are entire, and are greatly thickened posteriorly, where they support the posterior surface of the heads of the femora. In this position, when cut through, they present rather the appearance of fibro-cartilage than that of the ordinary ligament. Opening the joints freely by incisions through the thickened parts of the capsule, which retain

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their shape when cut, it is seen that the fibres pass to their normal pelvic attachments, passing between the heads of the bones and innominates posteriorly, and in front being strained over the site of the normal acetabulum. On the heads of the bones being turned forward it is seen that there is no ligamentum teres on one side, on the other a ligament much elongated and curiously flattened out and altered by pressure. The head of the femur on the side, without a round ligament, presents on its summit a leaf-like arrangement of fibro-cartilage, which can be raised on the handle of the scalpel, and which resembles the attachment of the corresponding ligament on the opposite side. It is clear from these facts that the process of absorption has been more advanced on the one side than the other, the ligament being all but removed on the one, removed completely on the other, except the few altered shreds at its femoral attachment. The old acetabula are filled with reddish Haversian bodies; and the bony outline, so far as one can trace it without maceration, is in great measure defaced. The heads of the femora are set on stunted necks, and are misshapen, while their axes are advanced (to use the description of Hutton) through an angle of more than a quarter of a circle.

A section discloses the fact that, though misshapen and maldirected, the osseous centre of the epiphysis of the head of the femur is present, and for the age sufficiently developed; the bones composing the innominates meet normally to form the acetabula, and show no defect along their triradial junctions. Looking at these facts, we may dismiss the idea of an arrest of osseous development as the cause of the dislocations. The condition of the capsules and even of the intra-articular ligaments similarly exclude the idea of traumatic dislocation, for in this injury, even where it has been observed in the infant, these structures have been found to have been ruptured. The appearance of the synovial membranes and surfaces exclude the idea that any synovial effusion or inflammatory change brought about the dislocations, for there is no sign in these joints of past inflammation, which, when sufficient to produce dislocation, leaves behind it ample traces.

I have said in my address—"I am certain that no one could

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tell what the cause is." I have now shown on this specimen that there are many assigned causes which we may exclude. I will not repeat the discussion I raised in my address, which has been already published.[a] I will only, before closing, remind the Section that there is ample reason to assert that the conditions which this specimen presents are, allowing for the age of subject, those constantly observed in this deformity.

[a] Dublin Journal of Med. Science, Jan., 1885.

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LARGE GALL-STONES PASSED PER ANUM.

By J. W. MOORE, M.D., Univ. Dubl., F.K.Q.C.P.;
Physician to the Meath Hospital and County Dublin Infirmary; Lecturer on Practice of Medicine, Carmichael College, Dublin.

[Read in the Pathological Section, January 16, 1885.]

On November 26, 1881, I exhibited to the Pathological Society of Dublin,[a] a set of large gall-stones, which had been passed *per anum* by an elderly woman, a confidential servant, whom I saw with Sir George B. Owens, in two violent attacks of biliary colic, in April and May of the same year. The attacks occurred at an interval of three weeks, and were characterised by agonising pain, only slight jaundice, obstinate and incessant vomiting, and intractable constipation. The largest calculus was of remarkable size, measuring three-quarters of an inch in length in its short axis and one inch in its long axis, two inches and three-quarters in its short and three inches and a half in its long circumference. The patient recovered for a time, but sank about a year after her illness.

On February 25, 1882, through the kindness of Surgeon-Major Hare, of the Army Medical Department, then stationed at Mooltan, in the Punjâb, I was enabled to submit to the same Society an account of three large gall-stones passed by a lady, aged forty-one, a patient of Surgeon-Major Hare.[b] The first was passed *per anum* on November 11, 1881, the second on November 25, and the third on January 24, 1882. This last was the largest. It measured a fraction over seven-tenths of an inch in its long axis, three-fifths of an inch in its short axis, and two inches in circumference. Periods of terrible suffering preceded the appearance of the calculi by four

[a] Proceedings of the Pathol. Soc. of Dublin. New Series. Vol. IX. 1882. Page 112. And Dublin Journ. of Med. Science. Vol. LXXIII. Page 151.

[b] Ibid. Page 120. And Dublin Journ. of Med. Science. Vol. LXXIV. Page 163.

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days in each instance, and Surgeon-Major Hare was of opinion that these paroxysms of agony coincided with the passage of the gall-stones through the common bile-duct. He wrote:—"No. 1 passed through the common bile-duct on the 7th of November, No. 2 on the 20th of November, and No. 3 on the 21st of January." In the first case, it was, on the contrary, more likely that the gall-stones reached the intestine by ulcerating through the adherent walls of the inflamed common duct and transverse colon.

In the autumn of 1884, my relative, Mr. Frederick Hone Moore, L.K.Q.C.P., L.R.C.S.I., practising at Anstey, in Leicestershire, kindly sent me a box containing seven gall-stones, among which are some of unusual size. They had been passed *per anum* by one of his patients, a woman, aged forty-seven or forty-eight years. By his permission, I exhibit them to the Academy of Medicine. The clinical features of the case, derived from notes furnished to me by Mr. Moore, are briefly as follows:—

Case.—M. C., a woman, aged about forty-seven or forty-eight years, very stout, of a sallow complexion but not jaundiced, suffered in March, 1884, from violent pains in the abdomen, and particularly in the region of the liver and gall-bladder. There was no vomiting, and her bowels were regular. The pains, which she attributed to approaching menstruation, passed off in some four days, when she did menstruate—in fact, had menorrhagia. The patient recovered and enjoyed very fair health, with the exception of occasional slight attacks of paroxysmal pains in the abdomen. In the beginning of July, 1884, the pains returned suddenly and with great violence, frequent vomiting of bile occurred, and there was also obstinate constipation. At this time castor-oil was repeatedly administered by the patient's relatives, but with no effect. Turpentine applied externally and hot fomentations gave some relief. When she was visited for the first time in this attack, Mr. Moore prescribed for her a drachm of sulphate of magnesia in half a pint of warm water every hour, but without effect. Fifteen minims of tincture of opium were then given every four hours, but this treatment only slightly controlled the pain. At night the patient's agony increased, and Mr. Moore was called out to see her. He then applied chloroform to the abdomen locally, with instant relief, and the patient slept until morning. The constipation

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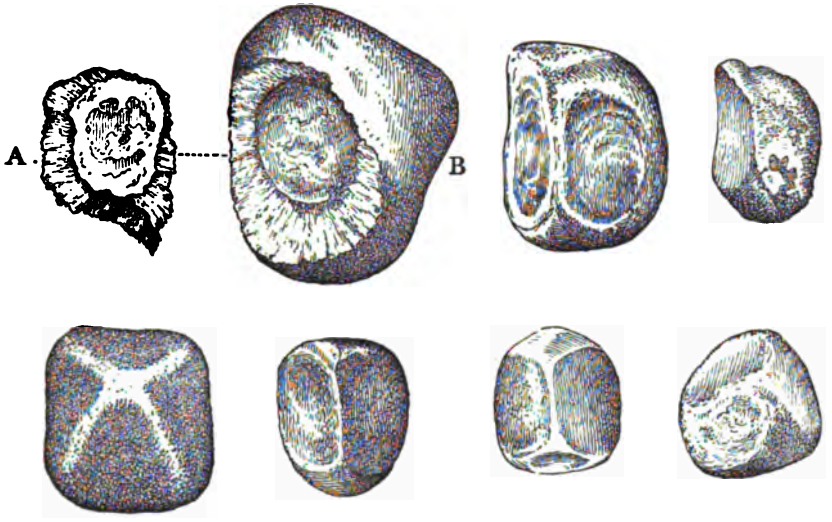
persisting, with pains and much nausea, Mr. Moore administered 40 grains of compound powder of jalap, with the result that the patient passed the largest of the calculi almost at once. Several other gall-stones subsequently came away, when she quickly recovered, and has since continued in fair health.



At my request, Mr. P. S. Abraham was good enough to make a careful examination of the seven gall-stones, which are represented of natural size in the accompanying woodcut.

Mr. Abraham reports as follows :—"Their composition is pure cholesterin. They weigh altogether 39 grammes (1 oz. 164·3 grains). There is one (marked A.—B. in the woodcut) much larger than the others, which weighs alone nearly 17 grammes (262·3 grains, or considerably more than half an ounce). This calculus is somewhat conical in shape, the base being smooth by facetation, and 31 millimetres (1¼ inches) in diameter. The height is 37 mm. (nearly 1⅜ inches). Except on the base, the surface is slightly rugose. At one side a large fragment (A.) has been shelled off, exposing at a deep level a flat facet, which from its rubbed appearance was probably exposed prior to the expulsion of the calculus. The other calculi present numerous facets, some of which are flat, others hollowed out."

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Apart from the size of these gall-stones, the question of the *modus operandi* of their escape from the gall-bladder into the intestine is of interest. It seems unlikely that such large calculi passed through the common bile-duct, and it is more rational to suppose that there was an attack of adhesive inflammation of the gall-bladder, followed by ulceration and perforation between that viscus and the intestine. The perforation probably took place at the time of the first severe symptoms in March, and was into the duodenum rather than into the colon, for these reasons—first, an interval of about four months supervened between the first and second attacks of violent pain; secondly, of these attacks the earlier may be assumed to be connected with the occurrence of perforation and the escape of the gall-stone or gall-stones into the duodenum, while the latter was due to obstruction in the lower bowel, being at once relieved on the expulsion of the calculi; thirdly, the intercurrent slighter attacks of pain were likely associated with temporary intestinal obstructions as the stones passed slowly along through the small intestines and through the ileo-cæcal valve.

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SELF-MUTILATION IN A LIONESS.

By P. S. ABRAHAM, M.A., B.Sc., F.R.C.S.;
Curator and Mem. of Court of Examiners, R.C.S.I.

[Read in the Pathological Section, February 13, 1885.]

On the 18th of May, last year, a fine lioness in the Zoological Gardens, in Phœnix Park, was discovered to have devoured, during the night, some six inches of her tail—the hair, skin, bones, and everything. She did not then touch it for some days, but appeared to be very restless, and on the 27th of the month she recommenced her extraordinary conduct, and demolished, during the night, a great part of the remainder of the organ. She then rested awhile, but again went at it, and at the end of a month there was nothing left of her caudal appendage but the "butt," some four inches or so, which I here exhibit. The organ was now so short and uncome-at-able that she could not reach it with her mouth, and it was hoped that in consequence she would resume her usual tastes and be satisfied with the flesh of other animals; but on the 1st of July she began to lick and gnaw off the skin of the dorsum of the right hind paw. The integument and subjacent tissues are seen to be removed from nearly the whole of the extensor surface of the foot, and it is evident that the tendons would be exposed were it not for the granulation tissue which has formed, as a superficial layer, over them. It was quite certain that while all this was going on the animal suffered extreme pain; the stump of the tail was seen to be in a constant state of quiver, and when a part of the foot was gone, the leg was drawn up, and the creature limped about the cage on the other three legs.

There was nothing apparently to account for this strange behaviour on the part of the lioness. She was in splendid condition, as regards her fur, flesh, and appetite, and her excretions were

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normal. It is needless to say, various methods were tried to induce her to leave herself alone—complete change of food, sulphur and other aperients, syringing the parts with bitter liquids, &c., but all with no effect. At last, indeed, it was deemed advisable to destroy her, for her suffering seemed so great, and the extent of the wound on the foot was so large, that, even if she left off the bad habit, it appeared impossible that the raw surface could ever properly heal and skin over.

At an examination of the body, made shortly after death, I found the thoracic and abdominal organs all perfectly normal; the right ovary was larger than the left, and its surface presented several large protrudent Graafian follicles. At first I was inclined to think that this ovary presented some abnormality, but after consultation with Dr. Neville, and on its microscopic examination, I have arrived at the conclusion that, beyond some degeneration, it is the seat of no very great pathological change. The brain and spinal cord were not examined.

The lioness, who was about twelve years old, had been in the gardens for five years, and had always been in good health. She had produced cubs three times, but her offspring were, with few exceptions, unhealthy, mostly becoming rickety and dying young. For one year previously to May, 1884, she had not been in season, although formerly she had been tolerably regular in "coming round."

It is well known that foxes and many other animals when trapped by one foot will sometimes gnaw themselves free and leave a portion of their bodies behind; and a gradual gnawing and picking away of the tail has also been observed as a not uncommon habit in monkeys in confinement, as well as occasionally in rats, dogs, and several other creatures. The present case, however, does not come under precisely the same category as these; for there appeared to be absolutely no external cause for the procedure, and instead of a gradual gnawing and disappearance of the organ, large pieces were scrunched off at intervals and swallowed.

I have made a great many inquiries as to similar occurrences in other zoological gardens and menageries, and I here take the

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opportunity of thanking several superintendents and others, some of whose names are mentioned below, for the information which they have been kind enough to give me. The distinction pointed out above does not seem to have occurred to my correspondents, many of whom have mixed up the cases of gradual gnawing with those more nearly like that of our lioness.

As far as I can find out, the only instances of self-mutilation which had previously occurred in our own gardens, were in (1) a female hyæna, who devoured her tail some years ago; (2) a female wolf, who fed upon one of her legs, and had to be destroyed; and (3) a female jaguar, who ate a good half of her tail, which had been injured by the claw of her neighbour, a tiger, over whose cage she was kept. One day, unfortunately, she let down her tail between the bars, and the tiger made a stroke at it; she ate the fragment, which was hanging by a little skin and tendon, but did not further interfere with the stump. This last case, indeed, is not exactly comparable with the others.

A keeper at the Zoological Gardens in London has informed me that some three or four years ago a young female cheetah, scarcely full grown, commenced suddenly to eat her tail. She would bite off two or three inches, then stop for a few days, then become excited and set to work again. This went on for some weeks, her excitement during the time being very great, and she frequently gave loud screams. Finding no improvement, she was destroyed. An old female hyæna, who had never bred, also some years ago at the London Gardens demolished her tail. She would wait till the stump was nearly healed, and then make another meal off it, until ultimately the whole organ disappeared.

Mr. Jackson informs me that the only case which has occurred at the Clifton Gardens was that of a lioness, a piece of whose tail had been bitten off by a neighbouring tiger. She kept the wound open and sore for two or three months, after which it healed over, and she subsequently became a good mother. This also is not quite in point.

M. Hüet, of the Jardin des Plantes, writes that such occurrences have sometimes happened in the case of leopards, lions, and smaller

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carnivora, and that they are especially common among the monkeys of that collection. He considers the habit due to a disease of the skin, which can be sometimes cured by treatment.

The late Dr. Bodinus, Director of the Berlin Zoological Gardens, had often observed animals of prey gnawing their tails and nibbling themselves; and he held that the bad habit has its origin in a faulty state of the blood. He recommended, therefore, a change of diet, such as from horse-flesh to young veal, and castor-oil in milk, also local applications, carbolic solution, tincture of aloes, &c. He stated, however, that it is very difficult to prevent wild animals from gnawing themselves when suffering from pain. He had nevertheless been quite successful lately with a young female jaguar who was eating her tail.

M. Herman, Director of the Amsterdam Collection, has only met with one instance of anything of the kind, in an experience extending over forty-six years. This was in the case of a rhesus monkey, "which, becoming mad, began to eat its forehands."

At the Antwerp Gardens no carnivore has ever attacked its tail, unless that organ had been wounded. More than once an animal has shown a tendency to constantly lick the tail, but a good dose of sulphur has removed the irritation. The monkeys, however, have frequently eaten their tails, and nothing but amputation of the organ has stopped them.

Mr. Jamrach has several times met with such cases among leopards and lions, and, of course, often with the monkeys. He ascribes the cause to be either a healing wound or an irritation of the skin, the intolerable itching developing into a mania. He states also that parrots frequently eat their feathers and flesh.

Mr. Salva, of Cross's menagerie, has frequently observed carnivorous animals mutilating themselves, especially among those which are suffering from scrofulous sores. He considers the only successful treatment to be by amputation of the injured limb.

Through the kindness of Mr. Snow, of the Phœnix Park Gardens, I have recently seen a letter from Mr. Carpenter, animal dealer, of Liverpool, who writes of a young Bengal tigress—"I had a letter last November (from the present owner) saying the tigress was very

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fine, but had a habit of dropping its tongue out and rolling it about a good deal; and a few days later I received a letter saying that it had eaten the whole of its tail off." This animal had never bred.

Lastly, Mr. F. Collins informs me that many years ago he knew of a lioness in Wombwell's menagerie who devoured her tail.

It will be observed that several of my informants ascribe the morbid appetite to some irritation of the skin; and, no doubt, this may account for the gradual disappearance of the tail in the case of monkeys. A sore may be originally formed at the tip, and, when the itching of healing begins, the scab is picked, licked or gnawed off; the newly rawed surface again heals over, again to be denuded; and this may go on till the organ at last is all gone. Of course a continuous licking with the prickly feline tongue will soon produce a sore, but it seems unlikely that this or the other suggested causes will account for an animal suddenly munching off large pieces of its person. On the whole, I am inclined to consider that this departure from the creature's usual habits is due rather to something akin to a mental derangement; and I think that M. Herman is probably right in saying that his rhesus had "gone mad." My inquiries tend to show that the carnivores which have "taken on" in this way have been nearly always females, which have been either very young, just before they began to breed, or old, at the menopause, when their breeding period had come to an end; and I venture to suggest that we may look upon this perversion of taste, in our lioness at all events, as one of the manifestations in the lower animals of that protean affection which we call "hysteria."

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GANGRENE FOLLOWING OCCLUSION OF A VEIN.

By W. I. WHEELER, F.R.C.S.;
Surgeon to the City of Dublin Hospital; late President, Royal College of Surgeons.

[Read in the Pathological Section, Feb. 13, 1885.]

The patient from whom this specimen was taken was a male aged forty-six years, who was admitted into the City of Dublin Hospital on Sunday, the 2nd of November, last year. He was perfectly well on Saturday morning, the day previous to his admission, but on the evening of that day he had his left leg twisted when wrestling with a friend. He was of intemperate habits, and by occupation a butler.

When seen by the medical attendant of the family in whose service he was, his leg was greatly swollen and discoloured, and his immediate removal to hospital advised. When admitted, in addition to the enormously swollen condition of his leg, large sanguineous bullæ were present, and much discolouration from black to light blue. The temperature of the limb was greatly increased. He was quite free from pain, and looked on the injury as not of much importance. The superficial veins were all much enlarged, even the small vein accompanying the peroneal nerve.

The specimen exhibits portions of the popliteal, posterior tibial, and anterior tibial veins, also the popliteal artery, the anterior and posterior tibial vessels, and peroneal artery. The specimen has been in spirit until the day before yesterday, when the veins were opened. In the anterior tibial vein and posterior tibial veins can be seen well-formed thrombi—the obliterating thrombi of Ziegler; in the posterior tibial vein may be observed the thinned coat of this vessel, interesting as showing how portions or the entire thrombus may pass onwards in the current; on the valves at junc-

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tion of posterior and anterior tibial veins may be observed a small thrombus—the valvular thrombus—and a little higher up, in the popliteal vein, what is termed the parietal thrombus. In this latter vein the rugous coats can easily be seen. The arteries have no special interest.

The most recent authority states that the blood which forms a thrombus is dead, and the coagulation takes place in two ways—either at rest, when the coagulum is formed from without inwards, when the regular red thrombus is formed; or in motion, when the chief part is played by the white corpuscles, when, if any red ones happen to be present, you have the stratified or mottled thrombus.

Bizzozero has described blood plates (*Blutplätchen*) which are identical with the hæmatoplasts of Haymen, and which Cohnheim has described as the third form of corpuscles found in blood. He says they cause coagulation, but there are two requisites necessary—first, that they should break up; and that they should break up in a fluid containing fibrinogen.

Two causes modify thrombosis—the alteration of the wall of the vessel, and the retardation of the current. In passing over diseased vessels, blood does not of necessity coagulate if the disease is limited, and if the blood immediately afterwards passes over healthy vessels. Hesitation in the flow (whether the vessel be diseased or the blood depraved) is one of the causes of thrombosis, for blood that is stagnant does not get the same "reviving" influence exerted on it as blood that is passing constantly from one part of the lining membrane to the other, and it is one of the few points in which physiologists agree that blood is prevented coagulating in the human body under ordinary circumstances by the influence of the intima. The thrombi here so beautifully formed, are undoubtedly caused from the depraved condition of the blood.

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ON A CASE OF CROUPOUS ENTERITIS.

By M. A. BOYD, F.R.C.S.;
Physician to the Mater Misericordiæ Hospital.

[Read in the Pathological Section, February 13, 1885.]

The patient from whom these specimens were removed was an agricultural labourer, aged thirty, married, three children, with a good family history, of temperate habits, there being no history of previous illness. He was admitted into the Mater Misericordiæ Hospital on the 4th of last December, suffering from a dysenteric attack, which was then of three weeks' duration. He traced the attack to standing in his clothes after a severe wetting, the results of which were a sharp shivering, cramps, pain in the head, constipation of the bowels, and vomiting, diarrhœa and bloody stools supervening subsequently to the administration of purgative medicines. At the time of his admission into the hospital he was emaciated, his pulse was weak and under 100, his tongue dry and brown, with incessant thirst, accompanied by soreness of the throat and difficulty of swallowing. His temperature was subnormal, his abdomen seemed flat, while along the left iliac fossa the colon could be felt through the abdominal wall, along its entire course, round, rigid, and tender. There was no tenderness at the region of the cæcum. There was slight tympany at the right hypochondrium. During twenty-four hours he had, on an average, from eight to ten stools, and the fæcal matter passed contained blood and mucus. There was tenesmus, and also a burning pain in the rectum, which was very troublesome. He had difficulty in micturating, and his urine was high-coloured, but contained no albumen. He had no cardiac or lung symptoms. Under a treatment consisting of 10 gr. doses of ipecac. with bismuth and hydrarg. c. creta, and enemata of haseline and opium, he improved, and his

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stools became less frequent, the blood and mucus disappeared, and the tenesmus became less troublesome. After a time, however, the dysenteric symptoms returned again, and his pulse became more rapid and weak. Vomiting and hiccough set in, and resisted all treatment, although his temperature all the time continued normal or subnormal. He sank and died from exhaustion six days after he came into the hospital, and about a month after the beginning of his illness. A *post mortem* examination was made twelve hours after death. The abdominal walls were found to contain little or no fat. There was no serum in the peritoneal cavity, and no sign of peritonitis. The ascending, transverse, and descending colon was thickened, shrunken, and livid. The veins of the omentum were greatly enlarged, and the mesenteric glands swollen and hard in the neighbourhood of the colon. The sigmoid flexure and rectum were thickened and fixed. The liver was apparently healthy, and there was no enlargement of the spleen. The kidneys were healthy, and there was no disease in the region surrounding them. An examination of the bowel showed thickening of the peritoneal coat and evident wasting and atrophy of the muscular coat, except at the rectum, where the muscular tissue was increased. The constant straining and tenesmus were the chief factors in producing this increase of muscular tissue from over-action. There seemed to be no disease in the cæcum. The mucous layer was enormously thickened through the colon from the cæcum to the rectum; but what I have to call attention to is not so much the thickening as the large deposition of adventitious membrane all over the entire surface of the large bowel. This deposition occurred all through the colon, and especially in the transverse position of it. There were no traces of ulceration. The adventitious membrane could be with difficulty separated from the bowel, and was quite flaky. It was evidently a croupous or diphtheritic exudation. It was united to the mucous membrane and continuous with it, and appeared to be a highly-organised new growth with a vascular network of vessels ramifying through it. The soreness of the throat and congestion of the pharynx, together with membranous patches distributed over it, showed a condition

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of things there similar to what occurred in the bowel. There was no symptom of hepatic disease. True tropical dysentery is rare in this country, and I am disposed to regard the majority of the cases described as such as examples of diphtheritic or croupous disease of the colon similar to this one, and the so-called patches of the mucous membrane passed, pieces of diphtheritic false membrane instead. The ulcerations of the bowel, with high fever, and symptoms of hepatitis and abscesses of the liver which are peculiar to tropical dysentery, were absent here. The subnormal temperature showed how little fever there was all through.

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A CASE OF PERICARDITIS AND CARDIAC HYPERTROPHY IN A HORSE.

By F. ALCOCK NIXON, F.R.C.S.;
Surgeon, Mercer's Hospital; Lecturer on Surgery in the Ledwich School.

[Read in the Pathological Section, February 13, 1885.]

A WELL-MARKED example of these diseases having come under my observation, I thought it would interest some of the members of the Academy of Medicine to examine the specimen, and to hear the notes which were taken by Mr. James M'Kenny, the well-known veterinary surgeon of Stephen's-green.

The animal, a bay gelding, sixteen hands high, aged, was placed under his care on 14th December, 1884, suffering from pleuro-pneumonia, which so yielded to treatment that he was discharged from his infirmary, in good spirits, eating well, but weak, on January 5th, 1885. On January 12th it returned to hospital with symptoms of great exhaustion; pulse 120, irregular; respiratory sound over formerly diseased parts normal, except at the inferior and posterior part of left pleura, where a friction sound remained. There was a friction sound also over the region of the heart.

The heart's sounds became dull, and subsequently there was a peculiar noise as of water dropping into a tin vessel. The animal refused all food. Mustard was applied over heart. Stimulants and diaphoretics were administered, and he was drenched with flax-seed tea and flour. Under this treatment he gradually improved, and continued to do so until January 21st, when he got suppression of urine. The pulse, which had come down to 60, increased to 100. He was ordered digitalis, spiritus ætheris nitrosi, flax-seed tea, and mustard over loins. Jan. 22nd.—Kidneys acting. 23rd.—Pulse slightly improved. 24th.—Serious relapse; pulse weak and quick, so that it could not accurately be counted; heart's

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sounds muffled and dull; dropping noise had ceased; died suddenly. On *post mortem* examination evidence of recent inflammation existed at posterior part of left pleura; the pericardium was enormously distended, containing fully four gallons of yellow fluid; lung in vicinity of heart inflamed.

Description of Specimen.—The heart and pericardium weigh one stone and a-half, the normal weight of a horse's heart being about six pounds. The right ventricle at its thickest part measures one inch, and at the apex $\frac{1}{2}$ inch. The wall of the left ventricle measures $1\frac{3}{4}$ inches, and also at apex $\frac{1}{2}$ inch.

Remarks.—The hypertrophy is doubtless of old standing, and appears to have been caused chiefly by the heavy work the animal was called upon to perform during life. The pericarditis, which is quite recent, appears to have been secondary to the pleuro-pneumonia, while the immediate cause of death was the enormous amount of fluid suddenly effused into the pericardium.

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SOME REMARKS ON A CASE OF MULTIPLE EXOSTOSES, AND THE PATHOLOGY OF THAT CONDITION.

By W. THORNLEY STOKER, M.D., F.R.C.S.;
Professor of Anatomy Royal College of Surgeons; Surgeon to the Richmond Hospital.

[Read in the Pathological Section, March 13, 1885.]

I WISH to submit to the Section some observations on certain forms of exostosis which involve the connective tissues in the vicinity of bone, and to exhibit portions of a skeleton showing them to a remarkable degree.

Virchow's division of bony growths into hyperplastic and heteroplastic, or those arising from the cartilage of bone or its periosteum on the one hand, and those originating apart from the skeleton on the other, is, perhaps, as accurate as such a classification can be, but is unsatisfactory from a morphological point of view, for there can be no doubt that all bony growths originate in the connective tissue group of structures. While, then, we correctly refer to the hyperplastic or homoplastic division those exostoses or enostoses which arise so commonly from the skull or bones of the extremities, we are less accurate in terming heteroplastic those osteophytes which are sometimes found in the lungs, skin, brain, or parotid gland, or in muscles and tendons; for, in these instances, it is the connective tissue of the structure invaded which is the original seat of disease. How inaccurate, from a structural or physiological point of view, this arbitrary division into classes is, may be seen by the different methods of arrangement pursued by various pathologists—thus, ossifications in muscle are sometimes called heteroplastic, and sometimes hyperplastic. That they belong to the latter group is clearly seen by examination of their commonest examples—namely,

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the "drill bone," observed in the deltoid of infantry soldiers, or the "cavalry bone," found in the thigh adductor of dragoons. These conditions are the result of the so-called myositis ossificans, are in their early stages confined to the connective tissue of the muscles, and are penetrated by canals or channels through which the as yet unaltered muscle fibres find their way. If these growths form connexion with bones it is obtained secondarily, and by an ossifying process proceeding towards the periosteum. In advanced cases of myositis ossificans such attachments to the skeleton are frequently found—as, for instance, in the remarkable case in the Museum of the University of Dublin (No. 837), or in that recorded by the late Dr. Bookey, the illustrations of which belong to the Museum of the College of Surgeons in Ireland. In the latter Museum will be found a number of bones, such as the scapula, No. Ea 116, exhibiting a similar condition.

In cases where the ossification is limited to tendons and other fibrous structures attached to the periosteum, a different order of events takes place, the ossification commencing in the formative tissue of the bone, and proceeding in an eccentric direction. Virchow has long since shown that, in connexion with ossifying periostitis, a parostosis, or development of bone outside the periosteum, may occur, and that in such cases bone production may spread by a sort of contagion to the neighbouring connective tissues. While it is, no doubt, true that, in the majority of these cases, some inflammatory action is a factor in the production of the outgrowth, most frequently, perhaps, of a syphilitic or rheumatic origin, yet I feel satisfied, from my observations, that they may occur in perfectly healthy persons, and without any inflammatory process as a cause. This presumption appears to be justified by an examination of two sorts of exostoses, of which numerous specimens exist in the Museum of the College of Surgeons in Ireland. In one set of examples the projections, as seen by section, are evidently periosteal growths beyond and apart from the normal bone, such as the femur, No. Ea 30, and the tibia, No. Ea 83; in the others, examinations fail to discover any line separating the projection from the neighbouring bone tissue, of which it seems to be an integral part; of this

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Fig. 1

Fig. 1a.

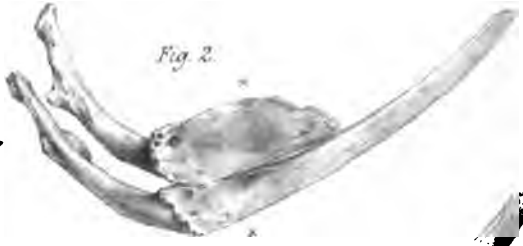

Fig. 2.

Fig. 3

Fig. 4.

Fig. 6.

Fig. 5.

Mr Thornley Stoker on multiple Exostoses.

1 & 1a, Right scapula. 2 & 3, Portions of 5th & 6th ribs of right side showing surfaces (×) for facet (×) on scapula. 4, 7th rib. 5, Lumbar vertebræ. 6, Dorsal vertebræ

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Fig. 5

Fig. 5a

Fig. 4.

Fig. 3

Fig. 2.

Fig. 1.

Fig. 4-a

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M^r Thornley Stoker on multiple Exostoses.

1, Left humerus, ant. view. 2, Right humerus, post. view. 3, Right femur.
4 & 4^a, Right tibia & fibula from before & behind.
5 & 5^a, Left d^o d^o d^o d^o

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condition the femora, Nos. Ea 12[2] and Ea 12[3], afford instances. I have seen growths of the latter class in persons who had always enjoyed excellent health, and who were free from any specific taint—in whom, in fact, I could imagine no cause, save an exceptionally active condition of osteal growth. I am strengthened in this belief by observing that the bones in these cases are usually very bulky.

I have had many opportunities of seeing these developments both in subjects dissected in the school of the College of Surgeons and in living persons; and, if sought for, they will be found more often than might be supposed. At present, I have two gentlemen under observation who have these parostoses in the situations in which they are most common—that is to say, projecting downwards from the inner side of the head of the tibia, and growing upwards from the lower part of the internal condyloid ridge of the femur. These conditions are usually symmetrical, and are found in the tibiæ and femora of the skeleton which is the subject of my text, and in specimens belonging to the Museum of the College of Surgeons in Ireland, Nos. Ea 12[2] and Ea 12[3] among others.

Injury has been set down as an exciting cause of nearly every form of osseous growth, including myositis ossificans; and, although it is difficult to understand why a local hyperostosis, the result of injury, should become general, yet such appears to be the belief held by most pathologists; and it is worthy of remark that, both in the skeleton in Trinity College and in that which I exhibit, evidences of old fracture are present.

The bones which form my text, and of the more remarkable of which illustrations are annexed, are now the property of the College of Surgeons. They were removed from the body of a large, muscular, male subject, aged about fifty-five years, which was brought into the dissecting-rooms of the College during the session of 1881–2. As usual, in the case of dissecting-room subjects, I could get no history, and the cadaver afforded no evidence beyond what may be seen in the skeleton. The parts present a striking family resemblance to the plate of Oscar Weber's case in Billroth's Surgery. Every bone in the body was more or less affected, was

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very heavy, unduly bulky, and exemplified the condition named by Virchow "leontiasis." There were no actual outgrowths on the skull, which I failed to preserve, but at the base it showed a good deal of exostotic roughness where the spinal muscles had been attached. From the appearance of the bones, particularly those of the spine and pelvis, I conclude that this subject had suffered from rheumatic disease, the pubic joint specially showing evidence of the "porcellaneous" condition; at the same time, it presents features unlike the ordinary effects of that complaint, notably, the great weight of the bones—the condition of "leontiasis"—associated with a normal smoothness of surface, except in certain situations.

The usual symmetry that may be found in multiple exostoses is very evident, each side being almost an exact counterpart of the other. The symmetry in such cases may be explained by the greater ease with which bone forms in the more important and well-defined masses of fibrous tissue, such as tendons, ligaments, and intermuscular septa, while it avoids, till later, those bony surfaces which give attachment to the less fibrous structure of muscle. This may be well seen in the ossa innominata of the bones in question, in which the iliac fossæ and those portions of the dorsum between the curved lines are comparatively smooth, while the crest, curved lines, and other points of fibrous attachment present remarkable changes. This holds good all through the skeleton; for, in every instance, a roughness or exostosis means a place where a lesser or greater bulk of connective tissue has an attachment.

This skeleton affords a good example of exostoses proceeding from the bones in an eccentric direction, and may with advantage be compared with the Trinity College specimen, in which the ossifications have evidently approached the bones from the muscles. The most pronounced enlargements have been selected for illustration, and are well shown in the accompanying plates. The bones were, without exception, as I have said, heavy, bulky, and possessed of abnormal development of every projection where a muscle or ligament formed attachment. In certain situations the

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projections were more extreme than in others, and in some places were so marked as to call for special mention.

Among these are the process on the venter of the scapula, and the surface formed by projections on the fifth and sixth ribs which articulated with it, the originally interposed muscle having disappeared. The projection downwards from the tubercle of the seventh rib is present in nearly all the costæ, and corresponds to the posterior edge of the external intercostal muscle. The vertebræ were, without exception, very irregular in their outline at every point of ligamentous attachment, and in many instances ossified together in groups of two and three. The enlargements about the femora are very marked and symmetrical, the spike of bone projecting upward so as to guard the opening for the passage of the femoral artery, to the frequency of which I have previously referred, being well seen.

The most notable enlargements, perhaps, and in my experience the most common, are those about the upper extremities of the tibia, that growing downwards on the inside of the head in particular calling for attention. The lower ends of the leg bones are considerably enlarged, and on the right side ossified together. The spike on the inside of the humerus is its most noticeable outgrowth.

In connexion with the ossification of tendons at their insertions, it may be remarked that they present a great likeness to the bony tendons of birds, and afford, perhaps no less than some of the changes which take place in the minute structures of our bodies, an evidence of the fact that disease is, in a sense, akin to a retrograde metamorphosis.

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TAR CANCER.

By CHARLES B. BALL, F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; University Examiner in Surgery.

[Read in the Pathological Section, March 13, 1885.]

Last session I brought under the notice of this Section of the Academy of Medicine the case of a man, named Curley, aged forty-one years, from whom I had removed a large epithelioma, situated on the front of the scrotum; and I may be permitted to recall the more essential features of his case. For nine years previously to the commencement of the disease he had been employed as a workman in Messrs. Bethell's tar works on Hanover-quay. I removed the cancer in November, 1881, it being then of eight months' growth, and, according to the history given, it had commenced as a wart, on which a hard horn grew, about an inch long. When I saw him first there were several small warts in the neighbourhood of the principal growth, upon which short horns were to be seen. Although advised to change his employment, he returned to the tar works, and, two years subsequently, he again presented himself at the hospital, and I removed a small epithelioma from the side of the scrotum, which did not, however, involve the operation cicatrix; and you had last session an opportunity of seeing a very beautiful microscopic section, made by our Secretary, of one of the small warts, with an attached horn. I have within the last few days seen this man. He still follows the same employment, and he told me he was quite well.

The second case, to which I wish more particularly to draw your attention this evening, is one of epithelioma of the back of the hand of an old man, who gave his age as "able to plant potatoes when the King was over here," from which I infer he must have been close on eighty. For fifteen years he had been employed distilling

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tar at Kürtz's works on Sir John's-quay. He noticed warts on his hands for the past seven or eight years, and about two years ago one of these, over the fourth metacarpal bone on the dorsum of the left hand, became sore and ulcerated, and has since gradually increased in size. In April, 1884, he was admitted to Sir P. Dun's Hospital, under the care of my colleague, Dr. T. E. Little, and he then had an oval ulcer on the dorsum of the left hand, measuring about two inches by one inch; it had a raised flat surface, with hardened edges, and on the exposed parts of the body—*i.e.*, the face, backs of both hands, and about halfway up the forearm, were numerous blackish horny warts; but none existed on the parts of the body usually kept covered. He declined to have any operation performed, and left the hospital. On September 1st, 1884, he was re-admitted under my care, when it was found that the ulcer had much increased in size, the extensor tendons and portion of the fifth metacarpal bone having been eroded; the lymphatic glands at the bend of the elbow and in the axilla were somewhat enlarged; there was frequent hæmorrhage from the ulcer, and the pain was so excessive that the patient was anxious for any operation that would give him relief. On September 11th I amputated the forearm, and recovery was rapid and complete. A microscopic examination showed the ulcer to be a typical epithelioma.

These two cases present many characters in common. The occupation of both patients was the same, and the disease commenced in both with warts, upon which hard epithelial horns were attached. The disease was of an essentially chronic form, and exhibited but feebly clinical features of malignancy. In the first case the glands in the groin, which were much enlarged and somewhat hard, subsided markedly after the removal of the scrotal growth; but in the second recurrence took place. The analogy between these cases and the chimney sweeper's cancer so ably described by Pott, is at once obvious, and although a similar form of cutaneous epithelioma has been observed in persons engaged in other occupations, such as gardeners, the Cornish smelters, who are exposed to arsenical fumes, and the operatives in paraffin manufactories, I have not met with any recorded cases where the disease

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has occurred to persons engaged in the distillation of tar, the reason probably being that the manufacture on a large scale of the many useful products from coal tar is comparatively a recent industry.

I lately visited Messrs. Bethell's works, and the manager, Mr. Stoer, courteously gave me all the information in his power. Only three principal products are manufactured in Dublin—a crude naphtha comes over first from the stills, then what is called creosote oil, and pitch is the residue of the process. The so-called creosote oil is a solution of carbolic acid, with many other products, and contains about 8 per cent. of phenol. It is used for creosoting timber to prevent it rotting, and is also exported in large quantities to England to be further rectified.

The total number of men employed in Dublin in this industry is 17—15 at Bethell's works, and 2 at Kürtz's, and of these two have been operated on by me; a third, named White, is the case alluded to by Mr. Story. I was informed by my first patient, Curley, that some years ago a man who worked at Bethell's yard had gone to one of the Dublin hospitals to have a sore, which had originated in a horny wart on his face, cut out, but he did not know to which hospital he had gone, or what subsequently became of him. In answer to my inquiries, Mr. Stoer wrote me that his foreman had recently a wart removed from his nose.

Although the number of cases I have to record is small, they bear a very large proportion to the number of men employed, and I think there cannot be any doubt that the direct exciting cause of these two examples of epithelioma was the occupation, especially when we consider their close resemblance to the soot wart, which is universally admitted to be the result of long-continued irritation—indeed it is quite possible that the same chemical agent is present in both instances; and we must bear in mind that this form of cancer is very uncommon in general surgical practice. The evident conclusion, therefore, is that some product of the distillation of coal and coal tar is capable of calling into activity latent neoplastic tendencies in persons who are for a long time exposed to its irritative action, and it becomes a matter of extreme importance to

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determine if possible the actual substance which produces these effects.

I recently asked the representative of one of the greatest manufacturers of carbolic acid whether any of the workmen there had suffered from warts on the exposed parts of the body. He replied, "Look here," and showed me his own hands with several warts and numerous small cicatrices where others had been. He stated that he had when a boy numerous warts, but they had disappeared for many years before his connexion with the manufacture of carbolic acid, and that they reappeared soon after he commenced his present occupation. He informed me that one man, whose duty it was to fill the bottles with pure carbolic acid, had his hands covered with them, and that several others of the workpeople were similarly affected; he did not, however, know of any who had cancer which required operation, although he said that sometimes when the warts fell out "proud flesh" formed, which they cured by burning with strong carbolic acid.

The occurrence of papillomata on the hands of adults is not very common, so that, if we believe the statement of this man, it would appear that the frequent exposure to concentrated solutions of carbolic acid acts as an exciting cause, and the transition of an indolent wart into a malignant infiltrating epithelioma is well known, and is exemplified in the two cases I have brought under your notice to-night.

The subject is one which requires fuller investigation; but the difficulties of obtaining reliable information are great. It would, of course, be premature to speculate on the bearing which this may have on long-continued surgical dressing; but I think a surgeon who is accustomed to the daily use of carbolic acid should view with suspicion the occurrence of warts on his hands. Possibly ten years hence we may know more about the matter.

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EPITHELIOMA OF THE EYELID, RESULTING FROM IRRITATION BY CRUDE CARBOLIC ACID.

By JOHN B. STORY, M.B., F.R.C.S.;
Surgeon to St. Mark's Ophthalmic Hospital, Dublin.

[Read in the Pathological Section, March 13, 1885.]

The occurrence of epitheliomatous ulcers as the result of chronic irritation has long been known to pathologists in the form of chimney sweep's cancer; but it was not, I believe, until Dr. Charles Ball made a communication on the subject in February, 1884, to the Pathological Section of the Academy of Medicine in Ireland, that anything was known about a form of cancer which there is reason to believe is induced by the irritation of the skin caused by long-continued working with crude carbolic acid.

Within a few days after Dr. Ball read the communication referred to, a patient presented himself at St. Mark's Ophthalmic Hospital with an affection of the eyelid caused by carbolic acid, and having the characters of a true epitheliomatous growth.

The patient was a man aged forty, employed in a manufactory of crude carbolic acid, at Ringsend Basin. The length of time which he had worked there was not noted. His right eye had been "blind" for sixteen years, from a cataract, probably of traumatic origin. He came to the hospital on the present occasion for advice as to his left eye, sight of which was becoming affected by a growth on or in the lower eyelid. The history he gave of this growth was as follows:—Four months previously he got a splash of "oil" on this eyelid, which caused an "oil-wart." On questioning him I found that the so-called oil was crude carbolic acid, and the "oil-wart," he told me, was a common result of its contact with the skin. He was quite familiar

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Mr John B. Story on oil wart.

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with the warts so produced, and had never heard them called anything else. He had had them frequently on his hands and other places, but had never had any real trouble from one before. I found, indeed, that he had an ulcer on the back of his hand, but its edges were not raised or indurated, and it did not, to my eye, present any malignant characters. This ulcer was better, though not healed, when he left the hospital some weeks later, and since then it has healed without noticeable cicatrix. The "oil-wart" of the eyelid fell off of itself (as he said they often did), but reappeared shortly afterwards in the same place, and this time its growth was accompanied by much swelling of the lid. The trouble it occasioned was so considerable that he consulted an oculist at one of the Dublin hospitals, who cut the wart off, eleven weeks before the date at which I first saw him. The removal of the wart gave but very transient relief, for it began to grow again, or rather the swelling of the lid began to increase a very few days after the operation. The wart proper did not return after this, but the swelling became steadily greater, till it completely prevented him doing his work by its interference with the sight of the only eye which was of any use to him. For the three weeks before he came to St. Mark's he noticed a swelling on the inner side of the lid. Up to that time it was only the outside that was swollen.

On examination I found the left lower lid red and swollen to about the size of a walnut. The tumour resembled considerably a suppurating meibomian cyst, which had opened on the conjunctival surface, and which involved the whole tarsus in its growth. Through the opening on the conjunctival surface a probe could be passed into the cavity of the tumour, and out through a small fistulous opening of the skin just below the ciliary margin. The skin round this opening was indurated and firmly adherent to the mass of the tumour. The rest of the skin of the lid was freely movable over the underlying mass. Through both openings a thin shreddy discharge could be squeezed out along with cheesy-looking matter. The external opening occupied the situation of the original wart.

The tumour both in its origin and its appearance was not like

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any growth I had ever seen in this situation, and I should have been at a loss to know what category to assign it to, but for the knowledge acquired from Dr. Ball's observations. Viewing it in the light thrown upon these so-called "oil-warts" by Dr. Ball, I could not but regard it as a tumour of epitheliomatous nature, differing in its origin and in its manner of growth from the epitheliomatous ulcers which commonly attack the eyelids. As, however, some doubt as to its malignant nature still remained, I resolved to give the patient the chance that local treatment afforded, especially as the whole lid would have to be excised if any operation were attempted, and I anticipated difficulty in obtaining sufficient covering for the cornea afterwards. Accordingly the cavity in the interior of the tumour was carefully syringed out daily for several days, and packed with chlorate of potash. I adopted this treatment because Dr. Charles Fitzgerald had stated at the Biological Club, that he had obtained satisfactory results from its use in the treatment of true epitheliomatous ulcers of the eyelids. The only result was to increase the internal cavity at the expense of the tumour, but not to diminish in any degree the total size of the swelling; the discharge, too, became more profuse, and the conjunctiva more vascular. I, therefore, gave up this treatment, and used powdered iodoform instead, but this latter produced no effect whatsoever. After some days the ragged ulcerated edge of the conjunctival opening began by its pressure to set up ulceration and infiltration of the cornea, and I could no longer defer operation without running great risk of complete destruction of the eyeball.

The tumour was removed by a V-shaped incision, preserving as much of the skin as possible. The wound healed satisfactorily in spite of some suppuration along the lines of incision. The corneal ulceration ceased, and so useful an eyelid remained that the patient was able some few weeks later to return to his work, hardly any trace of the operation being visible. At the time of operating it appeared likely that the whole of the diseased tissue was not removed, but as the microscope shows no epitheliomatous masses in the deeper lying portions of the tumour, and as I saw the man more than a

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year after the operation and found no return of the growth, this was probably not the case.

The eyelid, though small, is large enough to perform its ordinary functions.

The microscopical appearances of the tumour are those of epithelial cancer, as the sections which were kindly made for me by Dr. Denning fully demonstrate.

An interesting point in the etiology of this form of cancer, if we are to believe the statements of the patients as to its mode of onset, is that it does not appear to originate like chimney sweep's cancer, or the epitheliomata of the lips, from a long-continued chronic irritation of the cuticle, but from one isolated application of the irritant in question.

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COMPLETE DISLOCATION OF THE RADIUS AND ULNA OUTWARDS AT THE ELBOW-JOINT.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; Professor of Surgery, Trinity College.

[Read in the Pathological Section, March 13, 1885.]

The number of examples of this injury observed hitherto appears to be less than five-and-twenty. Of the entire series, the record does not contain a single *post mortem* examination in a case uncomplicated by fracture of the articular surface of the humerus; and, with this complication, only a single dissection, that by Professor R. W. Smith. I need not, therefore, offer any apology for submitting to the Academy of Medicine the dissection now before me of this rare lesion, entirely uncomplicated by fracture.

Before entering on its details, I may briefly refer to the recorded cases observed in the living. I think I have placed the number at twenty-five, well outside the limit. The first dates from 1816; the second, 1829; the third, 1842. From the able analysis of this third example by Nélaton we have the features of the injury defined, without, however, the verification afforded by dissection. I have here a reproduction of Nélaton's plate, enlarged to full size, from the small drawing in his *Pathologie Externe*. It absolutely represents the specimen before me in all essential details except one, while it gives, what I could not preserve in a dissection, the features of the deformity seen in the limb previous to dissection.

Beneath the inner border of the forearm at the elbow, when semiflexed, the humerus projects the skin and deeper structures. Through these one can see and feel the contour of the processes of the lower end of the bone, those on the inner side being of necessity the most prominent. Above the elbow on its outer side,

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behind and in front of the lower extremity of the humerus, the displaced bones of the forearm form projections, the olecranon behind the condyloid ridge of the humerus, the head of the radius in front of this line. These three tumours formed by the inferior extremity of the humerus, the olecranon, and the head of the radius, are the salient features of the deformity. The forearm is placed at greatest ease in the most extreme pronation.

I may now remark that every published record of the injury repeats these features almost without variety. Nélaton, Smith, Anger, Hamilton, are all alike; indeed, to this last writer, in his latest editions, we owe the most modern additions to the list of observations, but still there are no dissections. The illustration by Denucé requires separate attention in view of the details I propose to examine in my dissection.

I must mention Professor R. W. Smith's cases especially, because in the record of them, published in 1861, he claims the publication of the first dissection. Beyond doubt, his casts present the essential features of the lesion, but his dissection discloses a fracture detaching nearly one-half of the lower articular extremity of the humerus, which was displaced in company with the head of the radius. One may, then, fairly question which lesion should be regarded as primary—the fracture or the dislocation. The specimen I now show is not open to this doubt, and I may fairly claim for it the place of the first dissection of complete dislocation of both bones outwards.

I now pass to its essential features, which suggest many interesting questions. I regret that a total ignorance of the history of the accident which produced it deprives me of much that is essential to read its lines aright. I obtained it in the dissecting-room from the body of a female. The condition of the structures, both of the articular extremity of the humerus and of the false joint established on its external condyloid ridge, leave no doubt on my mind of the great length of time which must have intervened between the occurrence of the dislocation and the death of its owner. The opposite elbow-joint was normal, and the other joints of the body and the integuments and deeper structures carried no

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sign or trace of suppurative disease. I may, therefore, exclude from my present consideration both congenital and pathological dislocation. By exclusion, therefore, the dislocation is traumatic.

In the undissected body only one feature of the deformity raised doubt in my mind as to the nature of the specimen, and this rendered the exact diagnosis so doubtful that I did not take a cast of the joint. The examples in our collection taken by Smith were sufficient for the display of the external features, and the obscuring element in this was the large mass of bone which lay in the flexure of the elbow, movable on all surrounding bone, and of which the nature could only, in the absence of any life-history, be determined by dissection. Removing the integuments and superficial structure, I exposed and verified the displaced extremities of the radius and ulna, lying respectively in front of and behind the condyloid ridge of the humerus. The articular processes, with the epicondyle and epitrochlea of the humerus, form this tumour, projecting below the border of the forearm. These processes are entire, and only altered moderately by the effects of absorption acting through a long time. On the outside of the joint the external lateral ligament has been ruptured, and with it the attachment of the anconeus and the aponeurotic structure inserted beside it are all displaced and disarranged. On the inside the opposite condition prevails—the internal lateral ligament looks as if it had sustained no further injury than a change in direction of its fibres, as they followed the movement of the border of the great sigmoid cavity to its new position. I suspect, however, from their length, that though, perhaps, not torn across, they were much stretched, and many, possibly, ruptured. The ulnar nerve and the fibrous structure covering and embedding it, are normal except in position. The contrast between the inside and outside of the joint in these respects is very great. Next to be examined are the parts in front. The mass of bone which obscured the features of deformity is found to be a development in the deep surface of the fibres of the tendon of the brachialis anticus muscle. It is evident that the lower end of the muscle, with its tendon, was carried round the articular surface of the humerus, and probably in part ruptured, and was submitted to much strain by the displacement of the ulna.

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As happens in such conditions, as time advanced after the injury ossification extended through the tendon and muscle, and now formed a sort of bone cup for the anterior aspect of the lower end of the humerus. In the examination of these facts, one readily sees that the dislocation occurred with rupture and displacement from their attachments to the humerus of the fibrous structures attached to the epicondyle and its neighbourhood, while the forces limiting the displacement were the internal lateral ligament with its fibrous relations at the epitrochlea, and the brachialis anticus drawn backwards beneath the lower end of the humerus. The first idea that occurs to one in looking at this latter detail is, that the osseous mass is in reality the coronoid of the ulna detached from the lower limb of the great sigmoid cavity. I have spent much time in determining this question, and I am certain I have proof sufficient to set it aside, and to adopt the solution I have first given—that the osseous mass is a new growth. I have called it a mass—it were better named a shell; but we can easily see that its convex side, projecting in front, gives the first impression of a mass of bone.

There are two cogent reasons for rejecting the idea of fracture of the coronoid—first, the extent to which the fibres of the brachialis tendon can be traced all around to their normal attachment; and secondly, the existence of a distinct coronoid process unmodified by fracture, but altered in shape and direction by the effects of long acting pressure.

This leads me to the next point of interest in the case. The authorities, from Nélaton down, are very positive that an essential of this dislocation is the position of the olecranon and coronoid processes of the ulna embracing the external ridge of the humerus, the one behind, the other in front of it. Here this is not the case; the coronoid altered by pressure and time is placed with the great sigmoid cavity behind the ridge, while the head of the radius rests, as all are agreed, in front of the ridge.

This fact, taken with the condition of the internal lateral ligament, and the tendon of the brachialis, suggests a view slightly different from that hitherto given regarding the mode of production of the dislocation. The received theory is, that the displacement occurs in the direction of the transverse axis of the hinge of the

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elbow, the coronoid always keeping in front of the lower end of the humerus and its epicondyle, until a point is reached at which the supinator muscles, combining with the pronators, are able to cant the bones into a position of extreme pronation. This conception of the mechanism is plausible, and suggests itself naturally to one handling the dry bones while the movement I have detailed occurs. If the condition this dissection discloses be constant, another theory is necessary; we must admit that the dislocation is effected by an act of rotation of the bones around the internal lateral ligament, as the radius of a circle whose centre is at its apex. If on the recent joint we produce the dislocation experimentally, dividing the external lateral ligament, and then forcing the bones to pass outwards to the external limit of the articulation, the internal lateral ligament must give way; it does so by tearing clean off from the epitrochlea. I have shown that this has not occurred in the specimen before us. Again, in making the experiment as I have described, it becomes impossible to retain the brachialis in front of the humerus, it twists off clean to its outside; but in this specimen it has stayed in its place and acted as a powerful limiting agent in aid of the lateral ligament.

I cannot, of course, insist on these facts ae of value beyond the interpretation of the individual specimen, but we must recollect that none other has been dissected. The radius, at its lower end, presents an old Colles' fracture united with much deformity, and exhibiting in the articular surface the usual fissures present in the comminuted fracture. It is probable that this injury occurred at the same time as the dislocation of the elbow, but we cannot rely on it, in the absence of a life history, as evidence of the kind of force competent to produce the dislocation.

DESCRIPTION OF PLATE.

Complete Dislocation of the Radius and Ulna Outwards.

Fig. 1 exhibits the parts seen in this direction, while the bones of the forearm are supinated as far as possible. The dotted line at (*a*) marks the position of the coronoid and of the great sigmoid cavity which are in the preparation concealed from sight by the soft parts; (*b*) the deformity of an old Colles' fracture.

Fig. 2—A sketch after Nélaton; plate showing the sigmoid embracing the external condyloid ridge.

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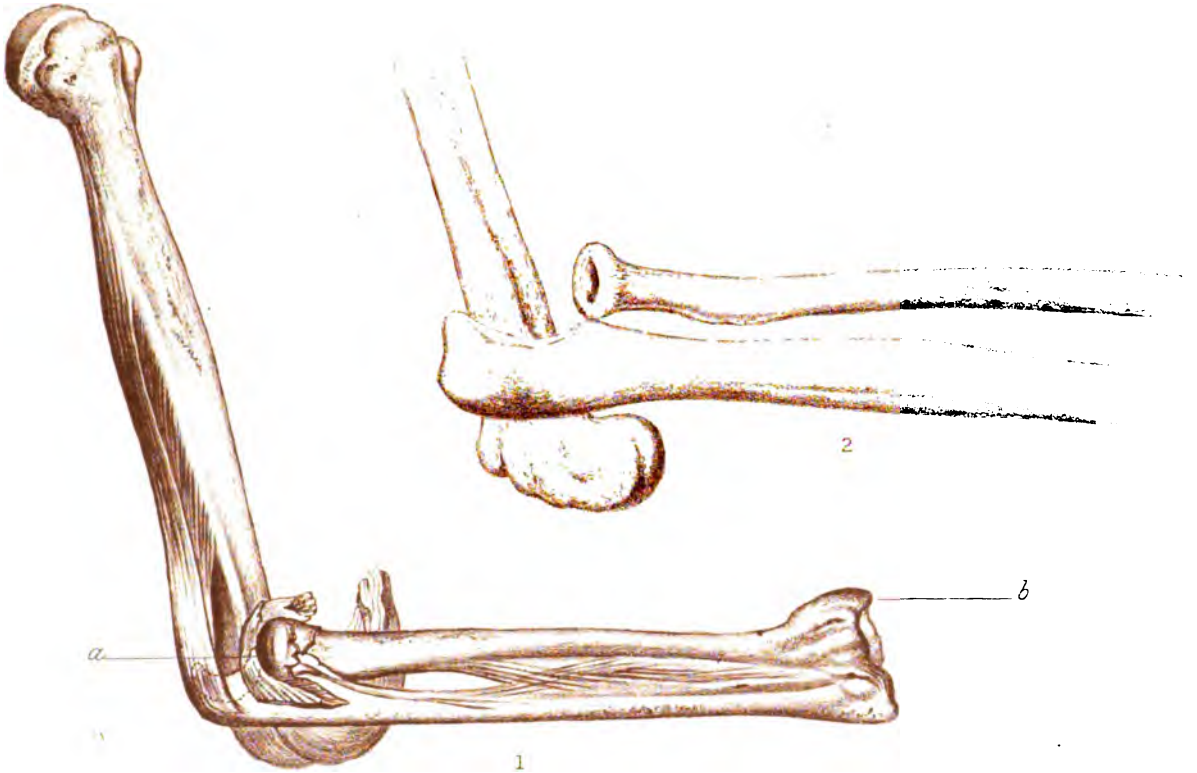

COMPLETE DISLOCATION OF THE RADIUS & ULNA OUTWARDS

DR E H. BENNETT

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DISSECTING ANEURYSM (SHEKLETON'S) OF THE THORACIC AORTA, EXTENDING TO THE TERMINATION OF THE COMMON ILIACS.

By J. MAGEE FINNY, M.D. Dub., F.K.Q.C.P.,

King's Professor of Practice of Medicine in the School of Physic, Ireland; Clinical Physician to Sir Patrick Dun's Hospital.

[Read in the Pathological Section, April 17, 1885.]

The specimen of aneurysm of the descending and abdominal aorta which I exhibit[a] would have little right to be the subject of any comments, or of a place in the Transactions of the Academy, were it not an example, and that a very typical one, of the dissecting aneurysm first described and figured by Shekleton in the *Dublin Hospital Reports*, Vol. III., p. 231, and that it presents many collateral points of pathological interest. Thanks to the kindness of the Curator of the Museum of the Royal College of Surgeons, I have here the two specimens which formed the basis of Dr. Shekleton's paper. As this specimen was taken from the body of a patient who had been under observation and treatment for a year and a half, it has an interest which was wanting in those of Dr. Shekleton which were taken from subjects in the dissecting-rooms of this College.

The following is an abstract of the clinical record of the case, which, however, I will anticipate by remarking that it is entirely silent on the subject of the dissecting aneurysm, and deals principally with the diagnosis and curative treatment of sacculated aneurysm of the abdominal aorta:—

J. W., aged forty-three, married (with healthy grown-up children), a tailor by occupation, was admitted under my care to Sir Patrick Dun's Hospital, on September 28th, 1883, with the symptoms and physical signs of aneurysm of the abdominal aorta. There was an

[a] This specimen is preserved in the Pathological Museum, Trinity College, Dublin.

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absence of all history of intemperance, syphilis, rheumatism, or injury to which it could be attributed, and, at the time of admission, and indeed for many months subsequently, the urine was normal and free from albumen. For two months before I saw him he had complained of pain in the abdomen and back, which, latterly, radiated both upwards and downwards, extending to the left groin and thigh, and was of so severe a character as to prevent his sleeping or attending to his occupation. He also stated that while walking he occasionally experienced a faint feeling, and had palpitation of the heart. He was of a bright, cheery, and contented disposition, and, except for an anæmic appearance, visible temporals, a slight "arcus senilis," and a strong rapid pulse (100), he did not at all look the subject of grave arterial disease.

Physical examination demonstrated the existence of a large tumour connected with the abdominal aorta, situated above its bifurcation. It measured 3½ inches long × 2½ inches broad. It lay above the umbilicus and a little to the left of the middle line, and conformed to the typical signs of a sacculated aneurysm—viz., distensile pulsation synchronous with the heart's beats, immobility to handling, change of posture, or deep inspiration; the presence of a systolic blowing sound over an area of two square inches, diminishing as the stethoscope was moved towards the ensiform cartilage or over the iliacs, and, lastly, the presence of a similar murmur in the back over the first three lumbar spines.

As the case was considered one well suited for Tufnell's modification of Valsalva's treatment, the patient was at once kept in the recumbent posture, placed on a regulated but liberal dietary, and given iodide of potassium in 5 and, later on, 7 grain doses. As he experienced relief to all pain and uneasiness in a very short time, the treatment was faithfully carried out by the patient for two months while in hospital, and after he went home for another two months. He was then readmitted for the purpose of examination of the aneurysm. When leaving hospital, December 15th, 1883, the dorsal murmur was no longer audible, and the sac of the aneurysm was very firm below, and at either side, though not above, and the murmur, which was faint and distant at the lower, was still distinct at the upper part. The clinical note made in March, 1884, was to the effect that the tumour was firmer and heaving rather than distensile; the murmur was not altogether gone, but was distantly heard, and confined to a small area at the upper left side of the aneurysm. The patient was now allowed to sit up for a time each

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day, and as, after four months' treatment and one month's subsequent observation, no relapse or change for the worse seemed to take place in the aneurysm, he was discharged cured at the end of April, 1884. He was soon able to walk about the streets slowly, and in June he went back to his work as a tailor, doing light jobs, without experiencing any return of abdominal uneasiness or dorsal pain.

It is right, however, to say that while the local disease was cured his general condition was one by no means free from anxiety, as he had passed through two attacks of pericarditis during the foregoing five months. Each attack was of short duration, and did not pass the stage of lymphy exudation; at the same time the heart had become increased in size, and showed signs of simple hypertrophy, though without valvular incompetency. From these indications the prognosis as to complete restoration to health was most guarded. How wise this opinion proved to be the sequel soon showed. In September he unfortunately undertook too heavy a job, and worked beyond his powers and against time, often till late at night. The strain proved too much, and he was struck down by apoplexy, followed by an incomplete right-sided hemiplegia and aphasia. For this he again came under my care in hospital. Two features of some interest were then present—one was the occurrence of albuminuria along with a few granular casts, showing that the kidneys were undergoing fibroid degeneration; and the other was the presence of well-marked Cheyne-Stokes' respiration, particularly on the patient lying low down on his back in bed.

From this attack of hemiplegia he never recovered, and, although he was well enough to walk about again and to leave hospital, he showed evidences of increasing enfeeblement of the motorial and psychical powers of the brain, and had a second attack in December which left him bedridden, and he died in hospital of asthenia on February 10th, 1885.

It is worth noting that during the eleven months subsequent to the time when the sacculated aneurysm was considered cured (March, 1884), there was never any return of pain or other evidence of a return of the disease.

Another comment I would make, and that is to the effect that

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there is no indication from the symptoms during the period of a year and a half during which he was continuously under my observation, nor in his history prior to the time I first saw him, as to when or how the dissecting aneurysm of the aorta commenced, or as to whether its presence in any way added to or modified the signs of the prominent saccular abdominal aneurysm.

A *post mortem* examination was made by Dr. Kiddle, our efficient house surgeon, to whom my indebtedness is great for the careful and complete manner in which the heart and aorta and iliacs were removed in their totality. It was confined to the thoracic and abdominal cavities. The lungs, liver, and spleen were healthy. The anterior surface of the heart near the apex was covered with a layer of organised lymph, the result of pericarditis. The heart itself was greatly enlarged, its increase in size and weight being due to simple hypertrophy of the left ventricle. The valves were competent; the anterior curtain of the mitral had, however, a small patch of atheroma near its basal attachment, and one of the aortic valves was slightly fenestrated. Subsequent examination of the parts removed revealed a condition of pathological changes in the arterial system, not alone entirely unexpected, but, viewed from the small amount of suffering and other subjective and objective symptoms during life, as most surprising.

The aorta, throughout its whole length, exhibited a beautiful example of atheromatous changes in its earlier stages—from the small, soft, red velvety spots to the scabrous, rigid, calcareous patches, though of these latter there were very few. Situated an inch and a half above the diaphragm, a circular opening (*A*) was discovered in the anterior wall of the thoracic aorta; it measured one and a quarter inches in diameter; its edges were sharply defined, the upper was thin and sharp, and the internal coat of the vessel was carried seemingly through it; the lower limit consisted of a bony plate which stretched quite across the calibre of the vessel. This opening led directly to an aneurysm, consisting of the outer and part of the middle coat, which was the starting-point from which a dissecting aneurysm of the aorta had its commencement. The dissection is both upwards and downwards—upwards for a distance

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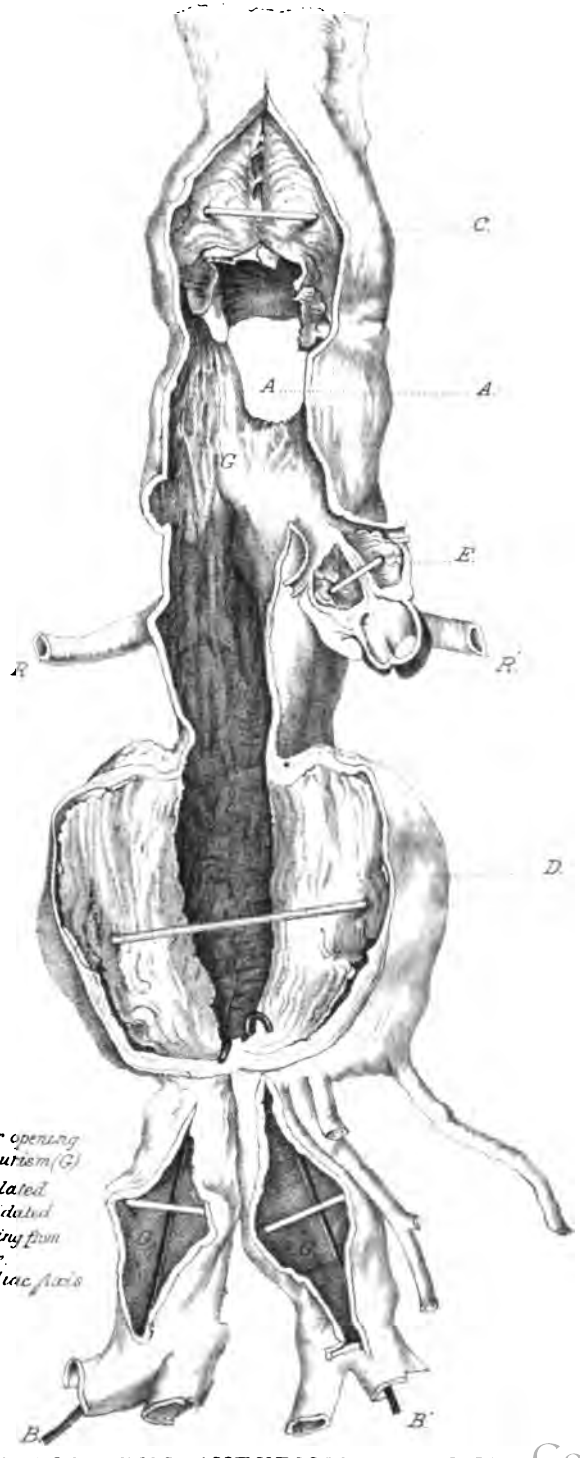
FRONT VIEW



A B. B.- Upper and lower opening of Dissecting Aneurism (G)
C D. - Secondary sacculated aneurisms consolidated by fibrine, springing from Aneurismal Tube.
E - Aneurism of Cæliac Axis
R. - Renal Arteries.

DISSECTING ANEURISM OF AORTA.

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Dr Finny

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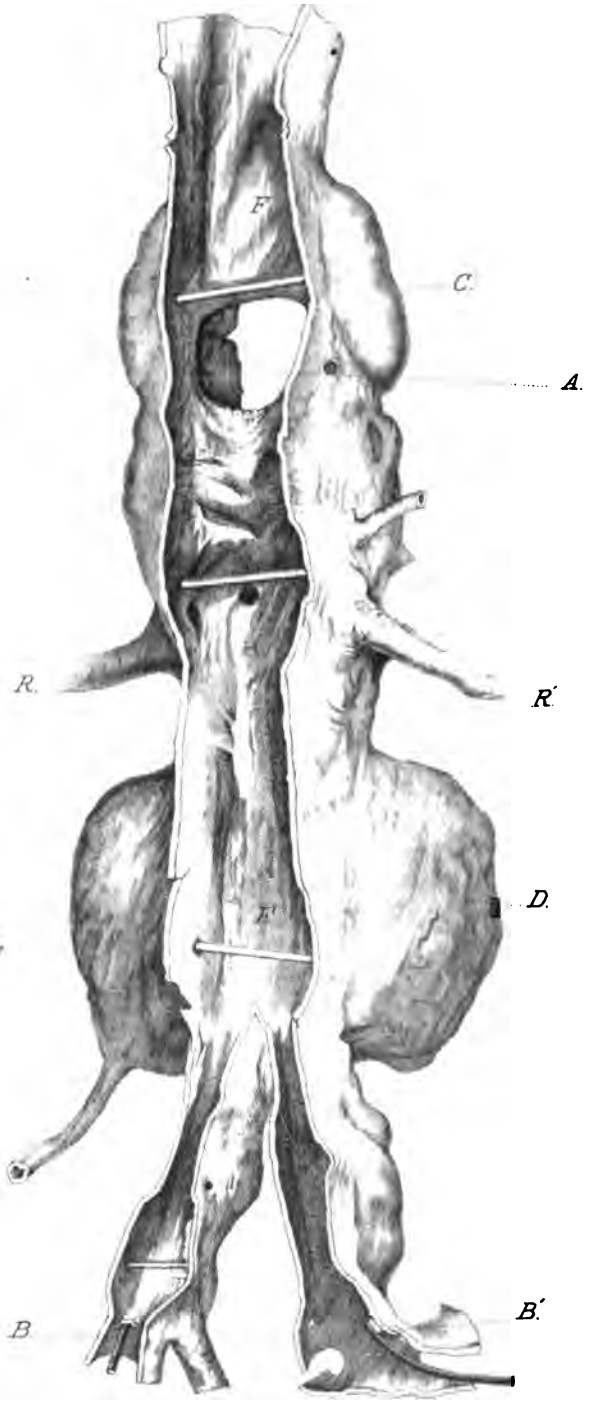
BACK VIEW



DISSECTING ANEURISM OF AORTA.

Dr Finny

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of three inches, where it terminates in a consolidated aneurysm (*C*) ($2\frac{1}{2}$ long × $1\frac{1}{4}$ broad). This sac lay in the posterior mediastinum, where it probably pressed on no important organ. The downward dissection (*G G*) occupies two-thirds of the anterior and right side of the aorta, and extends throughout its whole course, and, also, with the greatest symmetry, through both common iliac arteries to their termination (*A B B'*). It thereby produces an appearance as if these three arteries consisted of two tubes. This idea is further impressed by the fact that most of the branches which are usually given off by the aorta spring from the aneurysmal sac, the only branches from the normal channel being the superior mesenteric, left renal and the lumbar arteries. The inner surface of the aneurysmal sac, which contained a soft clot, is corrugated in parts, appears smooth and as if lined by a membrane in most of its course, and from it the arteries spring as if from the aorta itself. The sac is much larger in calibre than the aorta, which is very much narrowed throughout, while it is so constricted near the upper end of the aneurysm as to barely admit a No. 8 catheter. The iliacs at their commencement are also much narrowed. The two tubes communicate at the bifurcation of the iliacs (*B B'*), or rather at the point of junction of the external iliac artery on either side, by one small aperture through the lining membrane on the right side, and by two similar small openings on the left side.

Another and novel feature is the aneurysm (*D*) for which the patient came under treatment, and which was the only one which had been recognised during life. On opening the abdomen, it came to view at once, projecting forwards below the cæliac axis. It was very firm to the feel, and on section is a beautiful example of a firm, white, organised clot, into which the sac had been converted. Its coverings consisted of the outer coat of the aorta and part of the middle coat, except at its most prominent aspect. It measured five inches in circumference, and was three and three-quarters long by two and a quarter broad. It extended from an inch below the renal arteries to the bifurcation of the aorta into the iliacs, and from its left side were attached the inferior mesenteric and (?) left spermatic. The middle sacral vessel could not be found. Instead,

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however, of springing from the aorta, or being an aneurysm communicating directly with the interior of the aorta, it was found that it was a sacculated aneurysm springing from the anterior wall of the sac of the dissecting aneurysm already described. Its cause was not hard to explain; for, although there was an absence of atheroma to act as a factor, it was, doubtless, due to an extra weakening of the middle and external coats in this situation, and while the dissection was going on down the iliacs the anterior wall of the sac gave way in this particular region. That this was its formation is strengthened by observing a similar small pouching—sufficient to admit the tip of the index finger—which was beginning in the dissected portion of the right common iliac artery, and the unequal distension of false aneurysms is a feature of frequent occurrence. Around this aneurysm, and adherent to it near the spine, were several enlarged and calcified glands. The trunk of the cæliac axis was found to be converted into a solid mass, and on section, it was seen to have been the seat of a true aneurysm the size of a filbert nut (*E*). It was filled with a firmly adherent white coagulum which completely filled the aneurysm, and blocked up its two larger branches—viz., the hepatic and the splenic. How the circulation was conveyed to the liver and spleen under such circumstances is a matter of no little interest, and, though not verified by an examination of the vessels—since this aneurysm was only accidently discovered while preparing the larger aneurysms of the aorta—there can be no doubt that the coronary artery became the main conduit, and that, by the anastomosis maintained through the *pyloric* on the right and the *vasa brevia* on the left, the blood travelled in this circuitous route to these organs in sufficient quantity to supply them with nutrition and functional activity.

The kidneys were unaltered in size, but were examples of Bright's disease in the stage of interstitial nephritis. Professor Purser kindly examined sections of one, and found there was considerable increase in the connective tissue.

Remarks.—Dissecting aneurysms were recognised and described considerably before Shekleton's time. To Laennec the credit of priority has generally been accorded, but Dr. Peacock (Trans.

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Path. Soc., London, Vol. VI.), claims it for M. Maunoir, in 1802.

These aneurysms are usually described as of three varieties, or, more correctly speaking, degrees. The first, and that most frequently met with, and of which our museums contain many examples, is where the blood passes through a rupture of the internal coat of the vessel, and for a distance of one or more inches dissects the outer from the middle coat, or else separates the laminæ of the middle coat, and then finally either becomes a secondary false aneurysm in the areolar tissue surrounding the vessel, or else bursts into some of the neighbouring cavities.

When such an aneurysm is situated close to the beginning of the arch of the aorta, it not infrequently is the cause of sudden death, and the sac has been found to have ruptured into the pericardium.

The second, which may in many instances be but the early stage of the first, is that in which the blood, after making its way between the layers of the middle coat, seems to be arrested in its onward course, and may thus form a false aneurysm of limited extent.

The third, or that of Shekleton, is where, after separating the internal and external coats, or in the layers of the middle coat, the blood re-enters the normal channel through a second rupture of the internal coat. Shekleton describes it thus:—"The aneurysmal sac communicated with its own artery by a distinct opening at some distance from the original opening of the artery into the sac—thus, two channels for the blood into the artery beyond the aneurysm were established, the one through the canal of the artery, as in common aneurysm, the other through the sac."

The specimen I exhibit is an example of both the second and third varieties, for not only is it a beautiful specimen of Shekleton's, but from the aneurysmal sac spring two defined and well-marked aneurysms caused by the yielding of the outer coat.

Dissecting aneurysms are, comparatively speaking, very rare. In the Trans. Path. Soc., London, 1863, Dr. Peacock has tabulated and collected from various journals, every variety described up to that time from the year 1809, and the total number is but 80. I have

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carefully searched for additional recorded cases since 1863, in the Trans. Path. Society of London, and I find but 9 cases were presented during twenty-one years; while during the same period in the older Society, the Pathological Society of Dublin, I can find only 5, and in addition a beautiful specimen recorded by Mr. Knott before the Surgical Section of this Academy, in Dec., 1882 (Trans. Acad. Med. in Ireland, Vol. I.,), making the total, up to date, to be 95 dissecting aneurysms of all kinds. The *London Medical Record* since 1863 gives no additional example, nor has any aid been afforded by consulting Dr. Neale's multifarious Medical Digest.

I do not presume to say that only 95 cases of dissecting aneurysms have been met with and described during the last eighty years, but the unmistakable fact remains that such cases are of very considerable rarity. In the great majority of instances dissecting aneurysm occurs in the aorta in the first instance, though the separation of the coats of the vessel is not uncommonly carried beyond the aorta into its larger branches, such as the brachio-cephalic and the iliac arteries. The most common seat of origin—namely, the rupture of the internal coat, is close to—one to two inches from—the aortic valves. Of the 95 cases referred to, 79 occurred within the limits of the arch of the aorta. In 11 instances the primary rupture occurred below the origin of the left subclavian in the descending portion of the arch, while of the thoracic and abdominal portions of the aorta there were found but 6 cases. To this number this specimen adds a seventh.

It is further interesting to observe that of Shekleton's variety there are not more than 9 or 10 on record, while in extent and position there are but 3 or 4 cases which at all closely resemble those described by him. The longest course taken by the blood between the coats of the aorta and of its terminal branches is that of a case recorded by Dr. Swaine (Path. Trans., London, 1856, Vol. VII.), where the blood escaped close to the aortic valves through the internal and middle coats, and separated (dissected) them from the outer coat the whole way down to the right common iliac artery, where it re-entered the normal channel through a small aperture.

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Two other and almost identical cases were exhibited at the Path. Soc. of Dublin, in 1864—one by Dr. G. R. Symes, and one (by drawing) by Dr. Gordon, to whose kindness I am indebted for this drawing (drawing exhibited), and a third by Mr. Knott (already referred to), before the Surgical Section of this Academy, 1882. In these cases, as in that I now exhibit, the aorta and the iliacs seem as if they consisted of double tubes. In my case the aneurysmal tube is very much the larger, while, owing, doubtless, to this very cause, the original tube is much reduced in size.

The case again seems to be peculiar in its mode of origin and in its symptoms; for, in almost all the recorded cases in which the life-history was known, whether an atheromatous condition of the aorta be mentioned as being present or not, there is a certain moment when symptoms of a decided character occur, such as syncope, breathlessness, pain—often like *angina pectoris*, and from that instant till death a few hours or a few days are the measure of the duration of life. Doubtless, the period which elapses between the first symptoms of shock and those after reaction has taken place, and the mode in which death ensues, are alike mainly influenced by not so much the seat of the internal lacerations, and as to whether or not the blood can again re-enter the natural channel, as is the peculiarity of Shekleton's, but as to the way in which the primary opening of the sac occurred. Thus, in nearly every recorded instance the seat of the primary rupture of the internal coat is accurately defined, and the usual description of it is that it is marked by a rent which is either "linear" or "slightly ragged," or as if "cut with a knife." And in all these cases life was measured by hours, or, at longest, three months (Swaine).

On the other hand, in all the cases which have been accidentally discovered either in the *post mortem* room (as in my case), or in the dissecting room, such as the two by Shekleton and those by Symes and Knott, the aperture which primarily led from the aorta into the sac of the dissecting aneurysm does not conform to the above description, but is large and wide, and bordered by a hard if not calcareous edge.

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To the primary formation, therefore, of the dissection, as to whether it be a sudden rent through the internal and middle coats, or a gradual yielding due to atheromatous weakness, we are, I believe, justified in attributing the marked differences in the life-history and its duration in each class of case.

To point this more forcibly, I would note that in my case at no period was there any evidence to signalise the time when the dissecting aneurysm began, to indicate when the extensive separation of the outer coat was proceeding, and when the renal and inferior mesenteric arteries were cut off from the lining membrane of the aorta and communicated with the aneurysmal sac instead, or to show when the internal coat of the iliac arteries was ruptured to allow the blood again to reach its proper canals. This absence of symptoms is, indeed, most remarkable, and is but another instance of the extent to which pathological changes may take place and be unaccompanied by any constitutional disturbance or other symptoms.

I am disposed to consider that in this case, at least, the dissecting aneurysm was consequent upon, and subsequent to, the formation of a small saccular aneurysm of the thoracic aorta, and I would suggest the following as its solution:—

An atheromatous condition of the aorta throughout its whole course was the primary etiological factor, and caused a hypertrophy of the left ventricle. A couple of inches above the diaphragm there existed a large patch of atheroma which advanced to the stage of calcification. Here, just above this rigid patch, owing to the loss of elasticity the arterial wall yielded, and a saccular false aneurysm was formed, covered by the outer and either the whole or part of the middle coat—most probably the latter. The lower limit of the mouth of this pouch (*A*), which measures an inch across, stood out sharp and abrupt, and as at each systole the blood was forcibly driven through the aorta and against this rigid obstacle, the current was divided and part was directed upwards. The layers of the middle coat were thus more and more separated, and a dissecting aneurysm in an upward direction was produced, and extended along the anterior aspect of the aorta

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for a distance of two and a half inches (*B*) from the opening ring of the primary saccular aneurysm.

A similar process, and due to similar causes, took place at the same time or subsequently in a downward direction along the anterior two-thirds of the circumference of the aorta, and produced the aneurysm (*A B B'*), which extended with remarkable symmetry into the iliac arteries to their bifurcation.

The specimen has, however, another peculiarity which enhances it with further interest, and I cannot find any other exactly similar case among those published. I refer to the two circumscribed aneurysms which spring from the anterior aspect of the abdominal artery. One of these, the upper one (*C*), lay in the thoracic cavity close to the diaphragm, and was not recognised during life; the other lay in the abdomen, and was that which was recognised during life, and for which the patient was admitted to hospital and placed on a modification of Tufnell's treatment, and which proves to be a typical example of an aneurysm firmly consolidated by layers of fibrine.

The last point to which I would shortly call attention is, perhaps, the most interesting from a therapeutic point of view—and from which pathology must never be divorced—and that is, the complete and firm consolidation of the two sacculated aneurysms which sprang from the sac of the dissecting aneurysm, as well as of the third true aneurysm of the cœliac axis. In each case the sac is filled with a firm, decolorised coagulum of fibrine, which, from its stratified appearance in section, must have been deposited in successive layers, the oldest and most organised being that next the outer wall, the latest and less organised that washed by the blood. In the case of the aneurysm of the cœliac axis there is no channel through the fibrine, which completely blocks the vessel.

This is not the place, nor is it my intention, to discuss the general question of the treatment of internal aneurysms, but I now only remark that such specimens of consolidated aneurysms should encourage us still more to carry out the line of treatment with which the name of Mr. Jolliffe Tufnell, F.R.C.S., is justly associated, and which is now universally acknowledged to be the

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most reliable, if not the only, treatment for these aneurysms of internal arteries. For I look upon such treatment as that described in recent journals, of abdominal section, and inserting yards of wire into the sac of an aneurysm, as most unreliable, if not a very questionable proceeding. Many adjuncts, doubtless, belong to Tufnell's treatment, but its essential principle is to quiet the circulation by rest in the horizontal position (which may have to be maintained from two to four months), so that the fibrine may be deposited, layer by layer, from without inwards, and when so deposited may become consolidated and organised.

To insure a successful result there must, however, be another condition, on which Mr. Tufnell lays considerable stress. But, unfortunately, it is one which cannot be counted on with any degree of certainty until a trial of the treatment has been systematically made—and that is, that the outer wall of the aneurysm must consist of part, at least, of the arterial coverings. Where this vital covering does not exist, coagulation and fibrillation do not take place. To the absence of this covering, which receives its independent vital supply, is to be attributed the failure of the treatment in those aneurysms which, springing from the back or side of the abdominal aorta, cause absorption and destruction of the arterial walls by their growth and pressure against the vertebral column, or the ribs, and where consequently these structures (which are themselves continuously undergoing retrograde metamorphosis and absorption) form the posterior limit to the aneurysmal sac.

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SUBLINGUAL EPITHELIOMA.

By KENDAL FRANKS, M.D., Univ. Dubl.; F.R.C.S.;
Surgeon to the Adelaide Hospital, &c.

[Read in the Pathological Section, April 17, 1885.]

This specimen, which consists of a malignant growth about the size of an orange, was removed by operation, together with the middle portion of the lower jaw and the entire tongue, from a patient who was admitted to the Adelaide Hospital at the end of February last. The patient is a pensioner, aged forty, who had served for twenty-one years in the army, ten and a half of which were spent in India. He had always been a strong, healthy man, had never contracted syphilis, and had suffered from no serious illness, except ague, until towards the end of October last he noticed for the first time a "soreness" under the tongue. On examining this spot in a mirror he discovered that it looked red and irritated. He applied to a medical man, who prescribed a lotion for it, but this, he says, only "cracked it." Towards the end of December a hard lump appeared below and just behind the symphysis of the lower jaw. About the middle of January the skin over this spot became discoloured, inflamed, and very tender. Towards the end of January he found that the growth had extended forwards between the lower lip and the symphysis, that the lower incisor teeth had become loose and so tender that he could not use them at all. During all this time he suffered accutely from pain in the jaws, all the teeth ached, and flying neuralgic pains extended up the sides and back of the head, so that he was quite unable to sleep, and had lost flesh and strength.

When admitted to the Adelaide Hospital he presented a strongly-marked cachectic appearance, and he seemed to be suffering greatly. A hard tumour was felt extending from the lower jaw downwards

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to the hyoid bone, and the skin covering it near the middle line was of a dark, purplish hue and very tender to the touch. Inside the lip, and involving the gum corresponding to the central incisor teeth, was an elevated small mass, raw on the surface. The teeth in this situation were quite loose, and one of them fell out during his stay in hospital before operation. Behind these teeth and beneath the tongue was an elevated epitheliomatous patch about the size of a shilling. The tongue itself did not show external signs of disease, but on introducing a finger into the mouth the whole tongue felt deeply indurated, the left half being apparently harder and thicker than the right. He could not protrude it, any attempt at doing so causing great pain. The infiltration seemed to involve also the mucous membrane extending from the side of the tongue to the lower jaw nearly as far back as the anterior border of the masseter muscle.

His sufferings were so great that he urgently requested that an operation might be performed. Having explained to him fully the nature of the operation and the risks which he would have to encounter, without in the least shaking his purpose, with the concurrence of my colleagues I operated on the 5th of March. I began by making two curved incisions, convex downwards, from the centre of the lower lip to a point corresponding to the groove in the lower jaw on each side for the facial artery. This was necessary in order to cut the skin wide of the involved portion. The outer ends of these incisions were joined by a third, convex downwards, passing below the growth and reaching down as far as the hyoid bone. The lip flaps were then reflected back, and the lower jaw was divided on each side, just in front of the facial groove, by means of a small saw, the section being completed by means of a bone forceps. I then dissected the soft parts backwards, dividing the palato-glossi folds and all the other attachments of the tongue, until the front of the larynx was reached on each side. The root of the tongue was then detached by means of a pair of scissors. This process was accompanied by scarcely any hæmorrhage, the only vessels which spurted being the linguals and a branch of the submaxillary on the left side, and these were secured

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at once with Spencer Wells' forceps, and subsequently ligatured. One small gland was removed on the left side, but microscopical examination has since failed to show that it participated in the disease. As soon as the glosso-epiglottidean folds were divided, the epiglottis fell backwards, causing some distress in breathing, but this was easily remedied by passing a ligature through the middle fold and drawing the epiglottis forwards; subsequently these mucous folds were fastened by catgut sutures to the healthy tissues forming the new floor of the mouth. The lip flaps were brought together and accurately coapted by means of hare-lip pins. The other parts were united with catgut sutures, except in the middle line, where a somewhat quadrilateral-shaped opening was left—first, because they could not easily be brought together; and secondly, because it formed a most favourable position for drainage. The whole surface of the wound was dusted over freely with iodoform and then plugged with strips of lint.

The subsequent history of the case is shortly told. For two days he was fed with nutrient enemata, but subsequently a large-sized catheter was passed, at first by the Resident and in a few days by himself, whenever food was administered. The temperature twice only reached 100·4°. On the 20th, fifteen days after operation, he was up, and was able to go about the ward and make his own bed. On the 14th of April, that is the fortieth day, he was allowed to go home. His condition then was the following:—The wound had granulated up till the floor of the mouth had nearly resumed its ordinary level. The stumps of the inferior maxillæ on either side had become approximated, so that they were separated only by a narrow space; they had not united, as a small piece at the end of each had necrosed, but had not yet become detached. The neck wound had almost completely closed, and there was scarcely any discharge from it. There is not at present any appearance of a return of the disease, and the pain has almost entirely disappeared. He sleeps well, and complains only of a longing after solid food, which he is not yet able to swallow.

The specimen itself shows an extensive growth involving all the tissues underneath the tongue. A longitudinal section has

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been made through the tongue in its whole length and through the whole thickness of this tumour. When divided in the recent state, its hard, white appearance and its dense structure closely resembled a scirrhus tumour, but microscopical examination, kindly made for me by Mr. Abraham, shows that its structure is entirely that of an epithelioma. Radii from this growth extend upwards into the substance of the tongue, but do not anywhere reach its surface. This growth is continuous with the epithelial tumour in front, surrounding what was once the symphysis. This no longer exists, for the whole thickness of bone in this situation has been completely eroded, so that the jaw bone has been divided. The structure of the tumour, as well as its clinical history, show that this was the starting-point of the disease, and that the larger tumour beneath the tongue was an extension of this in an unusual direction.

I have brought this specimen before the Pathological Section of the Academy, as I consider that it is possessed of pathological as well as of surgical interest. The extension of an epithelioma, beginning at the frænum linguæ, downwards into the sublingual region, rather than into the substance of an organ so prone to the disease as is the tongue, is certainly an uncommon direction for it to take. It is also worthy of note that the malignant growth should have attained the size it did without involving the glands in the neighbourhood.

From a surgical point of view we can hardly expect that the disease will not recur, probably at a date not very remote, but the result which we have gained in this case shows that even such a formidable operation as the removal of a great portion of the lower jaw with the entire tongue and all the sublingual tissues down to the hyoid bone is more than a justifiable operation. The sufferer is relieved from the constant agonies of pain, sleep is restored, and life is more or less lengthened; and experience has shown that when the disease returns, and death ultimately ensues, this is due to a gradually increasing asthenia—a very different ending to that which follows malignant disease of the tongue when left to itself.

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ON SO-CALLED "MALIGNANT GROWTHS."

By GEORGE FOY, F.R.C.S.;
Surgeon to the Whitworth Hospital, Drumcondra.

[Read in the Pathological Section, May 15, 1885.]

HAVING recently had several cases of carcinoma under my care in the Whitworth Hospital, Drumcondra, and meeting with two possessing many points of similarity, I ventured to bring the subject of so-called "malignant" growths under your notice.

CASE I.—M'C., aged fifty-four, from Drogheda, where he was engaged as a hodman, is the case that I had photographed, and, as you may see, he is, with the exception of the lump, in excellent health.

The lump occupies the upper portion of the right side of the neck, has no attachment to the skin, and moves freely up and down, but not backward or forward, and gives the idea of a deep attachment. There is no history of cancer, phthisis, or syphilis. He is a married man, and father of a large and healthy family.

The tumour, when removed, was found to be, on microscopic examination by Dr. Gunn, an epithelioma. It implicated the submaxillary gland, and extended under the anterior bellies of the digastric muscles to the mental foramen of the opposite side. Its interior was semi-fluid. Some students, unfortunately, cut it up before I had an opportunity to weigh it, but the firm, thick wall of the sac weighed nine ounces. It was firmly adherent to the inner surface of the ramus of the jaw, and also to the internal lateral ligament, and pushed back to the spine, displacing the pharynx, and interfering with swallowing. Indeed, it was this interference that alarmed the man, and made him give his consent for its removal to Dr. B. Hamilton, late of Drogheda, to whom I am indebted for the case.

CASE II.—This was also an epithelioma, as the specimens, prepared by Dr. Abraham, and which are now under the microscopes,

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show. In this case there was no history of cancer, or phthisis, or syphilis. The man was about fifty years of age, and the father of a healthy family. He had suffered for years from lupus on the face, and, consequently, I could not secure a photograph. Some months before he came under my care an epithelioma was removed from his face, about the position of the right ala nasi. When he came to me he dreaded suffocation, and was suffering acute pain in his ears and along the occipital bone, of a neuralgic character. He was tormented by an excessive flow of saliva, and anxiously wished something to be done for him. After fully making plain to him the risk of the operation, I found him still willing, and I no longer hesitated to admit him to hospital. In his case, two large tumours, one on each side, occupied the superior half of his neck, interfering with breathing and swallowing.

After consultation with my colleagues, Drs. Wharton and Cosgrave, together with several other friends, including Drs. Montgomery, Lentaigne, Franks, and Gunn, it was decided to remove only one of the tumours at that operation; and I may mention, the vascularity was so great that thirty-five ligatures were applied, whereas in the first case of M'C. it was only necessary to apply five.

Though the wound did well, the man refused to allow the removal of the remaining tumour, and left the hospital. He died three months later, the remaining tumour having broken down, as I am informed.

The attachments of the tumour removed were many and deep, and the structure so brittle that it came away in bits; but, strange to say, as pointed out to me by Dr. Abraham, the sub-maxillary gland escaped the disease, though it was so completely embedded in the diseased mass that it was, of necessity, removed.

The frequency of these tumours, their recurrence on excision, their so-called malignancy, their supposed hereditary nature, and our present inability to treat them medically, make them a subject of the greatest interest. No social state is free from this terrible disease, and as "the race for life" becomes more difficult, so does this disease appear to spread more and more.

The many books and pamphlets on the disease, and the many nostrums puffed, both by the legalised practitioner and the advertising quack, for its cure, tell but too surely how unavailing have

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been our efforts to stay its ravages. In the "Bartholomew Hospital Reports," Vol. XIV., 1876, Mr. W. Harrison Cripps states, that of 2,379,622 deaths of persons over twenty in the ten years 1861–70, no less than 81,699 died of cancer; of these, 56,854 were females, and 24,845 were males—showing that females are much more subject to the disease than males. And Mr. R. Williams published, in last year's (1884) "Pathological Society's Transactions," statistics of 5,456 deaths from cancer, collected from the Reports of the Middlesex Hospital for seventeen years, and of the University and St. Thomas's for ten years. In two years, 1874 and 1884, 211 cases were admitted to St. Bartholomew's Hospital, and, in 1882, 154 were admitted to the Middlesex.

These statistics, imperfect as they are, show how widespread the disease is, and the many who suffer from it. As cancer is a disease of adult life it attacks the bread-winner, and just about the time when, if a married man, his labour and guidance are most needed by his family. Again, the greater liability of married women to the disease occasions the orphaning of a family of young children whilst as yet they have not passed through the diseases of childhood, and most need a mother's care. How great the consternation the appearance of cancer causes in a family must be known to all the members of the Academy. I have seen the affliction produce the blank, vacant stare that told of a suffering too great for either words or tears, and recalled to my mind the mother's face in Alma Tadema's "Death of the Firstborn," and I have asked myself how much of this pain is the result of a faulty medical name and theory—"malignant heredity"—a name which, to the lay mind, implies an inherent evil in the disease itself, and foretells a curse and a sorrow to the third and fourth generations.

I am inclined to the opinion that the prevalence of cancer is increased or diminished by the scarceness or plenty of food stuffs, and I am inclined to think that Mr. Haviland's views support this idea. What the effect of wars, famines, and such afflictions have on the increase or decrease of the cancer death-rate could only be told by a careful examination of the American and continental

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registrars' death reports for the past twenty years—a labour too great for an individual, and only to be undertaken by a society. Still, we have the example of Buckle to teach us how much may be done by well-directed investigations and great industry.

One thing is evident—namely, the disappearance of "cancer scroti" from the Hospital Reports, and altruism may be credited with stamping out this form of the disease.

Mr. Haviland has pointed out the prevalence of cancer in commercial centres, and Sir James Paget drew attention to the influence of mental trouble in predisposing to it. Dr. Snow, more recently, relates a case of cancer following on intense mental anxiety. Thus, I think, we may justly infer that privation and sorrow will increase the number of cancer cases. The frequency of carcinomas in the children of phthisical parents supports the theory that altered social conditions influence the spread of cancer.

Sir J. Paget's case of the transmission of the disease from the mother to two daughters and eight grandchildren is, probably, the strongest evidence in favour of the heredity of the disease; but this is a very exceptional case, as shown by Mr. Cripps, who obtained no history at all resembling it from 300 cases in St. Bartholomew's Hospital. Mr. Morrant Baker, who used much diligence, found distinct evidence of heredity in 47 cases, and, by examining into the family history of uncles, aunts, and cousins, to the second and third degree, traced heredity in 103 cases out of a total of 460 ("Bartholomew Hospital Reports," Vol. II., 1866). Dr. Snow, of the Cancer Hospital, Brompton, commenced life in the hospital with a firm belief in its heredity; but, in his "Clinical Notes," published in 1883, he declares himself totally opposed to this idea, and gives an account of 81 cases in which he traced heredity out of a total of 734, or about 11 per cent.; of these, 358 were mammary cancers, with 54 showing heredity; 268 uterine cancers, with 23 evidencing to heredity; and 108 were epitheliomas, with 4 giving an hereditary history.

In the Middlesex Hospital Report for 1882, a peculiar fact may be noticed—namely, that of 31 uterine and 41 mammary cases, there is no history of the father having suffered; and in 38 mammary

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cases 2 mothers had had disease, and of 31 uterine cases 4 mothers; but when we come to examine the records of cancer of the tongue, we find that in 1 out of every 17 there was a history of the father having suffered, but in none of them did the mothers show any history of cancer in the 25 cases.

This record is of much interest, especially when we remember Ribot's statement—that the daughter inherits from the father, and the son from the mother. Yet, here we have 25 sons suffering from cancer and no trace of cancer in the mothers, and 72 daughters cancerous and no trace of cancer in the fathers. To me it seems a fair induction from this table, that similar habits and occupations produce many of the effects we ascribe to heredity.

The daughters of an untidy mother are seldom tidy—not so much from heredity as from example, and the father that is an inveterate smoker sets an example likely to be followed.

Common report supports the idea of the hereditary transmission of cancer, and, knowing how all sores difficult of healing were called cancers, we can understand how easily a history of cancer in a family could be made out.

The name "malignant" was at first used in medicine to designate an evil spirit who caused convulsions in children, and, to facilitate its expulsion, the "ghostly" adviser scraped a hole in the child's skull; and, in time, the name became a synonym for cancers, because the primitive savage saw an external sore which was not amenable to treatment or exorcism, and ended fatally. Not understanding natural phenomena, and not seeking out natural causes, he flies to the supernatural for explanation, and concludes that the spirit of an offended ancestor has made the patient his victim, and that neglected funeral rites were being avenged by the eating ulcer, in which resides a malignant spirit. Such ideas, promulgated for many centuries, influenced our greatest minds, and the most vigorous intellects could not rise above their educational bias.

To quote a recent writer:—"Only as societies grow, arts multiply, experiences accumulate, and constant relations of phenomena become recognished, registered, and familiar, does the notion of natural explanation become possible. Only then can there arise

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even scepticism respecting these earliest conclusions. Only then can there begin the slow process of replacing them."

The name "malignant" is no more suitable to cancers than any other disease. If it be taken to imply a bad prognosis, and if it be intended to suggest a peculiar inherent evil, I consider it wrong; and I hope to show that this name, so alarming to lay minds, is unsuited, and should be discontinued. I must confess that it is with much diffidence, and a keen desire for information on the subject, that I bring forward my views.

The exciting cause of the disease is, in the great majority of cases, some irritation that, under other conditions, would produce either hypertrophy or simple inflammation; but now taking place in the decline of life, or in an organ whose functional activity is exhausted, or in a part weakened by previous injury, it produces the initial changes that lead to hypertrophy; but the evolutionary changes are either not regulated normally, or are in abeyance, and the effects of surrounding influence predominate over the effects of inherited tendencies.

Probably, the simplest of these tumours is an epithelioma of the lower lip, and its most frequently assigned cause is irritation by a pipe; yet the man has, probably, smoked for years and no evil resulted until in the decline of life, when cell metamorphoses and specialisation are slow of production—nay, he, if a day-labourer, may have suffered from extensive inflammation of the lips—as harvestmen very frequently do, and still the result be a restoration of the normal condition on the subsidence of the inflammation.

And on reading Mr. Hey's 10 cases of cancer of the penis, in which 8 were ascribed to congenital phimosis, we must see that, in two of them at least, the supposed exciting cause had long been present; in the tenth case the man was 76, and in the third case 63, and, until the vitality became lowered, the irritation caused no serious annoyance.

Mr. Butlin, no doubt, gives two cases of cancer occurring in boys—one in a child of two years old, and the other in a child of six years old, but they are not given with sufficient fulness to justify an induction.

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Normally we find protecting epithelium produced by fission of the deeper cell layer that rests on the cutis vera, and the cell, as it gradually comes to the surface, becomes flatter, drier, more scale-like—becomes specialised as protecting epithelium. And if we examine the growth of a hair we notice an ingrowth that produces the hair bulb; in both cases the first step is cell multiplication and growth, and, at the same time, commences a series of changes that cease only when the cell is specialised.

And this power of specialisation is characteristic of the higher animal and vegetable kingdoms. Animals and vegetables low in the scale of organic beings are homogeneous and unspecialised. The amœba and Alpine red snow may be quoted as examples.

What the power is that regulates the necessary changes we cannot even guess at. If we did not know that the nervous system is itself the product of such changes we might infer that it is the prime factor.

But what occurs when an epithelioma is being formed? The irritation causes a hard little kernel to form, which, on section, exhibits the well-known "bird's nest" arrangement, the cells showing sufficiently well that, at the beginning of the trouble, a hypertrophy was attempted, and failed; and, deeper still, we come on nests of cells but little altered from the cells of the deepest layer of normal epithelium, unless the tumour is of long standing, and fulfilling the functions of homogeneous animal plasma—growing rapidly, multiplying quickly, of unsatisfiable appetite, and omniverous; eating their way amongst the surrounding tissues, feeding on the softer tissues by preference, and, if transplanted to a distance by accident, as in Krashe's case, as reported in the *Medical Record* of last month, or by the lymph current, the embryonic-like cell exerts a disturbing influence on the part, and fresh cancer growths occur.

But all irritations in advanced life do not produce cancer. If there remains sufficient regulating energy to produce heterogenity in the cell contents, the functional activity of the plasma is diminished—it has undergone a metamorphosis; and exactly in proportion to the directing evolutionary influence, so will the

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resulting growth possess the characters of an adult tissue, or embryonic tissue growth.

I have not spoken of tumours as belonging to any special embryonic layer, because the layers are specialised embryonic tissues, and I consider that all tumours have an identical genetic origin, and that the characters we consider as evidences of "malignancy" are simply the normal physiological action of embryonic cells not becoming specialised. Rapidity of growth and multiplication is not due to malignancy, but is the normal activity of low organisms. Dr. Hooker cut the leaf of a begonia into one hundred parts, and each piece produced a whole plant. Their spermatic influence they share in common with the pollen of flowers. Their rapid decay is due to the central mass becoming too far removed from the food supply, and their infiltration of surrounding tissues is due to their preference for the softer varieties of tissue; to use an illustration, they become imbedded in their stroma, as men become located in coal mines, from their picking out and removing the material they seek to the neglect of the remainder.

I have made no mention of congenital cancers, of which Mr. Bryant gives two examples in "Guy's Hospital Reports" for 1875, Vol. XX., 3rd series; but if my theory has any truth, no congenital disease should be more common; any arrest in specialisation of the embryonic cells would result in a tumour. And Mr. Bryant, referring to these congenital cases, says—"That cancer may occur before the fœtus has an independent life, is suggestive of some fault on the part of the parent; and yet, in the cases given, no hereditary history of a cancerous taint could be obtained. That it appeared when cell-growth was the most active was suggestive of a local origin, the cell itself, from some want of developmental or controlling power, expending its force simply in cell multiplication. These views also receive support from the cases reported of cancer attacking the bones of infants."

I think the fœtus need not have a cancerous diathesis, but may suffer from cancer if deficient in that power which regulates the cell changes from simple fission to the most differentiated of the tissues.

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ON TWO SPECIMENS OF SARCOMA OF THE LOWER JAW.

By P. S. ABRAHAM, M.A., B.Sc., F.R.C.S.;
Curator and Member of the Court of Examiners, Royal College of Surgeons in Ireland.

[Read in the Pathological Section, May 15, 1885.]

At the request of Dr. P. C. Smyly, I exhibit to the Pathological Section the accompanying two specimens of new growths of the lower jaw.

The first was excised from an army veterinary surgeon, aged twenty-five, who for six months before operation had complained of severe pain and increasing swelling of the lower jaw. The symptoms commenced while he was on service with his regiment in India, and they were at first ascribed to the constant champing of the chain-strap of his helmet. The acute pain, however, did not subside, nor did the enlargement cease to increase when the supposed cause was removed; and as the diagnosis of a tumour implicating the bone became evident, excision of the jaw was recommended. The operation was effected under ether. The growth was accompanied by no glandular enlargement. It appears to have been completely removed on the right side, but the cut surface of the bone on the left has not a healthy appearance. The specimen shows that the neoplasm occupies the substance of the mental part of the inferior maxilla as far back on the right side as the second bicuspid tooth, and on the left side as far back as the first molar. It has expanded the bone forwards, downwards, and backwards, forming a tumour (7 cm. in vert. diam., and 5 cm. in ant. postr.), which must have been felt in front and below the chin, as well as within the mouth beneath the tongue. The surface shows some irregular lobation, and it seems to have been encapsulated by the surrounding expanded tissues.

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A clean vertical cut has been made nearly through the middle, and, meeting without resistance, proves that in this situation the bone has practically disappeared. The section exposed a firm, fibrous-looking structure, with a large irregular central cavity.

Microscopic sections exhibit the anatomical structure of sarcoma, the mass of the growth being made up of connective tissue cells of embryonic type. They are, for the most part, spindle-shaped, but they are often rounded, or sometimes irregular; and an intercellular matrix is occasionally well developed. At various spots throughout the sections, the remains of bony spicula have been cut across, as though the new cellular growth had expansively separated, as well as absorbed, the bony lamellæ.

A section taken through one of the lobes which encroached on the gum inside the mouth presents the same characters; but it also exhibits some hypertrophic development of the epithelium, and some neighbouring collections of inflammatory small cells.

The pathological question arises as to the source of origin of the cells of this sarcoma. Have they their parentage in the cells of the osteogenetic periosteum, from the internal medullary cells, or from the minute protoplasmic elements which, as bone cells, occupy the lacunæ of the bony substance, or are to be found along with the vessels and membranes which traverse the Haversian canals? In the present state of knowledge it appears to me to be impossible to give a definite answer to this question. It has been proved by experiment and observation that, in a certain sense, "irritation brings bone back to an embryonic condition," that the first effect is an absorption of the osseous tissue, and that this is closely followed by a new formation of bony trabeculæ, so that the ultimate result may be a dense and thickened bone. Suppose, however, that this accompanying ossification be arrested, that the multiplying cells in place of acting as osteoplasts, grow quickly, increase in number rapidly, and keep on their primitive character, we should have just such a growth as the one now under consideration—a true sarcoma, studded with the vanishing remnants of the old bone. The rapid growth of this tumour, its homogeneous soft texture, and the complete absence of large plates of the old bone, as well as

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its microscopic characters, all seem to me to point to such an interstitial mode of origin.

The disease returned in a short time in the left side, and grew rapidly. This was removed at the articulation, and the wound healed readily; the other side was quite healthy. On the 8th May there were further signs of disease between the larynx and œsophagus, and extending towards the floor of the mouth.

The second specimen is also a tumour occupying the front of the lower jaw. The case is that of a lady, aged thirty-five, who had noticed the growth for one year. In consistence it is very hard, and portions, for microscopical examination, could be sliced out only with difficulty. The sections exhibit the appearance of an ossifying sarcoma; in many parts, however, the cells of the matrix have assumed an "adult" character, and these portions resemble rather a young fibrous tissue. The bony trabeculæ are well developed, and off-shoots of bone extend outwards, as though from the periosteum, towards the integument of the chin. That the seat of origin of this growth is in the periosteum appears to me, indeed, not improbable. While the first described tumour may be regarded from its structure as a growth of highly malignant character, this may be considered an example of one of those slow-growing sarcomata which clinically closely approach the innocent tumour.

In this case there has been no return, and the wound is perfectly healed. There was a small abscess under the chin, about the fifth week after operation.

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NOTE ON TUBERCLE OF THE TONSILS.

By P. S. ABRAHAM, M.A., B.Sc., F.R.C.S.;
Curator and Member of the Court of Examiners, Royal College of Surgeons in Ireland.

[Read in the Pathological Section, May 15, 1885.]

The specimens exhibited are the enlarged tonsils of a young woman, aged twenty-eight, which were excised within the last three months by Dr. P. C. Smyly, who informs me that the patient was unmarried, had a strumous appearance, and swollen glands in the neck, but no symptoms whatever of lung disease, cough, or any affection of the palate or fauces. She suffered from repeated slight attacks of tonsillitis, but there was never suppuration. For some months before the operation the tonsils had become so enlarged, meeting in the middle line, that the throat was nearly occluded, and only small quantities of fluid could be swallowed with difficulty. The wounds caused by the operation healed readily and without trouble.

The right gland, which is rather the larger, is attached by a narrow base. It is roughly ovoid in shape, and measures 4·5 cm. in length, and about 3·5 cm. in its thickness and in its width. The surface is marked by indistinct lobation, and several fissures and pits are seen to lead into the depth. It was smooth, and of pale pink colour in its recent state. A longitudinal incision made into the substance exhibits a few deep crypts enlarged at the bottom, and much solid hypertrophied tissue between them. Some spots of the latter were yellowish, and softer than others, from apparent commencing caseation, and some cheesy detritus also partly occupied some of the crypts.

The microscopic sections show—(1) peripherally, many hypertrophied and succulent epithelial layers; (2) hypertrophied lymphoid

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follicles, which with (3) make up the mass of the growth; and (4) a loose, irregular cell growth, situated chiefly between the latter, and markedly characterised by the presence of well-developed multinuclear giant cells—a tissue, indeed, which differs in no essential point from that of ordinary tubercle, as found elsewhere. In some places the centre of a follicle is occupied by a giant cell, with a loose zone of young connective tissue cells immediately around it, and in other parts the cells are evidently undergoing caseous degeneration. A few sections were stained by Gibbs' method for bacilli, and although there is some doubt, possibly in consequence of understaining, I believe that the bacillus tuberculosis is demonstrable in the giant cells, and especially in their nuclei.

The sections taken from the tonsil of the other side present all the same characters.

Although some authors admit that a connexion has been observed between enlarged tonsils and the strumous diathesis, in the principal British works on diseases of the throat no allusion whatever is made as to the possibility of the tonsils ever being the seat of tubercle. Wagner, in Von Ziemssen's "Cyclopædia," incidentally mentions tubercle of the tonsils as occurring with tuberculosis of the soft palate; and he states that it only occurs simultaneously with advanced tuberculosis of the other organs, especially of the lungs, &c. Some pathological authors, however, do recognise its existence, although, as far as I can discover, the occurrence of tubercle primarily in the tonsil is nowhere particularly noted.

The adenoid tissue of the lymphatic glands is, we know, a favourite site for tubercle, with its resulting caseation; hence, we might, *a priori*, expect it sometimes to appear in and among the lymphoid follicles of the tonsil. The present specimen seems to me to be an example of such a condition; and it may be predicted that tubercle will again, occasionally, be found in this situation, when the habit of microscopically examining all excised tonsils shall have become more general.

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CERVICAL LYMPHO-SARCOMA.

By M. J. KILGARRIFF, F.R.C.S.;
Surgeon to the Mater Misericordiæ Hospital.

[Read in the Pathological Section, January 16, 1885.]

I HAVE the honour to exhibit to the Academy a portion of an enormous cervical tumour, removed by me, in the Mater Misericordiæ Hospital, from a young woman aged twenty-five years.

Six years previously a growth was removed from the same site by the late Mr. O'Leary, M.P. Six months afterwards it recurred, and continued to increase up to the date of the operation—early in November last.

As may be seen by the photographs [exhibited], the tumour implicated the lower portion of the ear, the face, over vertical ramus of lower jaw, extending, at this level, two inches behind the ear. The entire tumour was irregularly pear-shaped, with the base below overlapping the clavicle, and, in this situation, measuring six inches transversely.

The lower portion rested on the sheath of the common carotid, and the upper portion was embedded deeply in the parotid region. I shall deal with the operative and clinical features of the case before the Surgical Section.

Six months prior to admission an ulcer formed, several existing when she came under my care. The bleeding from these ulcers reduced her strength considerably, her face and lips presenting a blanched appearance on her arrival in town.

The tumour was carefully examined by my colleague, Mr. Coppinger, whose report I append :—

"Tumour pear-shaped, smooth on the surface, and encapsuled, the fresh section showing large areas of translucent substance, from which mucin exudes on pressure.

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"Microscopical sections of these portions of the tumour presented the usual appearance of myxomatous tissue. The other parts of the growth are made up of lymphoid tissue, with a scanty reticulum, and numerous cell elements of varying size and shape—some large and multinucleated, and others of smaller size, arranged in groups or masses.

"The tumour seems to be a lympho-sarcoma, large portions of which have undergone myxomatous changes."

I may add, the patient made a rapid recovery, perfectly free from deformity, presenting merely a short cixatrix following the curve of the upper and back part of lower jaw.

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EPITHELIOMA OF TOE.

By M. J. KILGARRIFF, F.R.C.S.;
Surgeon to the Mater Misericordiæ Hospital.

[Read in the Pathological Section, January 16, 1885.]

I REMOVED this toe, last November, in the Mater Misericordiæ Hospital, from a man, aged sixty-two. Two years ago the disease appeared. On his admission the matrix presented a villous appearance, and exuding a brownish fœtid discharge, and occasionally bleeding copiously.

My colleague, Mr. Coppinger, who kindly examined the specimen, reports:—"An epithelial ulcer, papillated and villous on surface; tubular-shaped masses of cells are traceable down through the deeper layers of the corium to the subjacent periosteum."

The unusual site of this disease is my excuse for occupying the time of the Academy.

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REPORT OF COMMITTEE OF REFERENCE.

By P. S. ABRAHAM, F.R.C.S., Secretary.

The following specimens have, at various times during the session, been referred to the Committee of Reference, and placed in the Secretary's hands :—

a. A calculus, removed from the tonsil of a gentleman, by Mr. F. A. Nixon.

b. A mammary tumour, removed from a girl, aged twelve years, by Mr. J. K. Barton, December, 1884.

c. Sections of tumours appearing after exposure to irritation from carbolic acid, March, 1885.

(*a*). The tonsillar calculus weighs 4·5 grammes, and is of an irregular shape, 23 mm. in its longest diameter, and 17 in its shortest. Parts of the surface are rugose, and irregularly mulberry-like, but at one end a remarkable saddle-shaped, smooth facet is situated, 13 mm. long, and 9 mm. broad, probably caused by the play over it of the palato-glossal muscle. At one side of the calculus is seen an irregular depression, 11 mm. in diameter, and about 4 mm. in depth, with very rough walls. A section of the calculus has been made, and exhibits but little evidence of lamellar aggregation; the texture is seen in some places to be freely porous, or occasionally more spongy, while at other parts a denser formation is apparent. The powder collected from the sawing becomes gray on heating, showing the presence of some organic material, but its bulk is only slightly diminished by prolonged heating. It effervesces slightly with acid, proving the presence of CO_2, and dissolves nearly completely. No uric acid could be demonstrated by the murexide test. Its main composition is, probably, phosphate of lime, with a little carbonate.

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(*b*). The Committee has adopted the description of the minute structure of Mr. J. K. Barton's mammary tumour, from a girl, aged twelve years, which was given in the appendix to Mr. Barton's paper.

(*c*). The sections of Messrs. Story and Ball's carbolic acid tumours show the structure of ordinary epithelioma—epithelial cells in collections, and nests, and in exuberance, occurring heterogeneously in the deeper tissues of the parts.

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SUB-SECTION OF STATE MEDICINE.

SEWER-GAS AND DISEASE IN DUBLIN.

By E. MacDOWEL COSGRAVE, M.D., L.K. & Q.C.P.I.;

Hon. Sec. Dublin Sanitary Association, Physician to Whitworth Hospital, Drumcondra.

[Read in the Sub-Section of State Medicine, February 5, 1885.]

During the past few weeks a lengthened correspondence has been carried on in one of the Dublin dailies, as to the best means of banishing rats from a house, and various remedies have been proposed—some intended to slaughter the intruders, others to render their entry difficult, and others to act on their fears, and so prevent their return.

Very little attention, however, was paid to the most important point that, whilst the presence of the rats themselves in a basement is, at most, intensely disagreeable, the fact that they can have this access is strong evidence that there is a direct communication between the interior of a sewer and the interior of a house, and that sewer-gas can enter with ease.

It is not, however, only houses in which rats appear that are in danger, as sewer-gas can escape through slight cracks and imperfections in sufficient quantities to do considerable harm, as when fires are burning a house is like a chimney with a strong up-draught, to supply which thousands of cubic feet of air must enter per hour, which, replacing air, will, in accordance with Nature's economy of exertion, enter wherever there is least resistance. In the basement of a house, when the doors are shut, there are few legitimate openings, consequently the suction is great; and if there is any direct communication—no matter how small—between the interior

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of the drain and the interior of the house, sewer-air is certain to enter. Indeed, it is not uncommon for air to rush up through rat-holes, or defective or badly-set traps, with sufficient velocity to extinguish a candle.

This evil is not so great in Dublin as in other places, because the street sewers are well ventilated, but it is not removed. Sewer-gas, can, it is true, no longer collect at a pressure, and force its way past water-traps; but, independent of the suction of fires before alluded to, on damp, foggy days the external atmosphere is much heavier than the internal, and so, if there is a passage between the sewer and the house, the current of air will tend in that direction. Indeed, the outer air is generally denser than that in a house. If a built drain run under a house, sewer-gas is almost certain to find entry. The drain is large enough to harbour rats, and they can pierce its walls, and junctions are seldom accurately made; such a drain is also badly flushed, and is, probably, old and dilapidated.

The danger of sewer-gas entering is lessened if a pipe-drain with properly cemented joints be laid, and if all its confluents are water-trapped; but it is not gone, as the only obstacle to the entry of sewer-gas through any branch is a water-seal of, at very most, two inches in depth, which water-seal is far from being a perfect safeguard, for the following, amongst other, reasons:—

1. If water stays very long in a trap it becomes foul, absorbing sewer-gas at one side, and, perhaps, giving it off into the house at the other.

2. Sewer-gas, at a pressure, may force its way through the trap, or internal suction may draw it through.

3. If a house is empty for some time, or if any house drain is left unused, the water will evaporate, and the sewer-gas have free passage, and to allow this free passage it is only necessary for the level of the water to fall below the middle partition.

4. Traps as a protection for overflow pipes are practically useless, as, unless the ball-cock be out of order, no water overflows, and so the trap is dry; and if, as often happens, the overflow pipe opens into a soil pipe not disconnected from the sewer, the sewer-gas will pass up freely, and be greedily absorbed by the water in the cistern.

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5. Traps may be badly fixed. Often the trap only opens over the drain pipe, and is not fastened to it. This is especially met with in sculleries. In other cases the setting of a trap is defective, as when in a midfeather trap the water-seal is perfect, but the partition is not accurately joined to the roof, sewer-gas passing over it with ease.

6. Traps can be discharged by "syphoning." Sometimes the discharge of water from an upper closet sucks the water from the trap of the closet below.

7. Traps can be interfered with. Once, when examining the drains of a house in which there was a case of enteric fever, I found a trap in the cellar floor from which a decidedly bad smell was escaping, and, on calling the attention of the servant to it, was informed—"Yes, indeed, it is a dirty thing; when I wash the cellar out it always remains damp, and I have to take the water out with a sponge."

8. It must also be remembered that such traps give a feeling of false security. In another case of enteric fever I found every drain trapped, save that in the cellar, and it opened directly. If there is any fault in the drain between the trap and the sewer the trap is comparatively useless.

So that, although all drains should be trapped, the trap should not be depended upon to exclude sewer-gas, and further precautions are necessary. The best of these are to cut the house drain from the sewer by an intercepting trap, and to promote constant circulation of air in the house drain by having openings from it to the outer air at different levels at each end. If, in addition, it is occasionally flushed with a sudden and copious stream of water, and if the pipes are properly laid and joined, the house will not only be safe from sewer-gas, but even from any drain smell.

The effects of sewer-gas on health are, in many cases, easily traceable. There is often intestinal derangement, with occasional attacks of diarrhœa; soreness of throat, headache, and increased temperature are also common. After a time anæmia sets in, and sometimes enlargement of the lymphatic glands; there is drowsiness, disinclination for exertion, careworn appearance, and other signs

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of lowered vitality. The symptoms, in short, point to a double causation—the sewer-gas escaping from the drains acting partly as a direct poison, and partly by leading to an altered state of the air, which is rendered comparatively inert, and incapable of maintaining the body in a healthy condition, the proper amount of oxygen being, indeed, present, but only in a devitalised condition.

The symptoms are most likely to develop first in servants, especially in those sleeping in the basement story. Indeed, an inquiry into the health of the servants is often a great assistance in doubtful cases.

New-comers are also most likely to show symptoms, as the system can become so accustomed to unhealthy surroundings that a family may live for years in a house with fair health, whilst those who succeed them are rapidly stricken down.

But, unfortunately, these general symptoms seldom attract attention early enough, and an outbreak of enteric fever, of diphtheria, or of illness in the puerperal state, or after surgical operations, is required before an inquiry into the condition of the drainage of the house is insisted upon.

A short time ago the Dublin Sanitary Association—perceiving the necessity that existed for some system by which a skilful and unbiassed report on the sanitary condition of a house could be obtained at a moderate and known charge—started a Sanitary Protection Department for the benefit of its members. Although this Department has been so recently organised, a large number of houses have been examined. Some of the results I will now briefly lay before you, first acknowledging my indebtedness to the Engineer of the Association, W. Kaye-Parry, M.A., C.E., &c., to whom much of the success of the Department is due, for many of the facts I am about to lay before you.

The first case is chosen in order to show the importance of having the system of drainage examined before going into a new house. The following cases show the co-existence of sanitary defects with disease presumably arising from those defects:—

Case I.—A gentleman was in treaty for a house in one of the North-side squares. It was guaranteed to be in perfect sanitary

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order, a new w.c. having been put in, and a pipe drain laid within the previous few weeks—all being done under the personal supervision of the landlord. I was requested to give my opinion, and, on examination, finding things far from satisfactory, advised that Mr. Kaye-Parry should be consulted. This was done, and he discovered the following condition of affairs:—The top of the old brick drain, which was choked with sewage, had been removed, and a line of pipes laid within it without previous removal of the filth; the lengths of piping were not in any way cemented together, and allowed free escape of fluid, which kept the sewage moist; the pipe drain ended at the coal-cellar door, the original brick drain being used from that to the street sewer; the pipe from the servants' w.c. was a 6-inch one, and opened at the scullery door into a 9-inch pipe, no taper length being used; the portion of the 9-inch diameter not filled by the 6-inch pipe was open, and allowed the sewer-gas to escape freely. Another fault was that the Vartry water-pipe from the street main was also laid in the old half-choked sewer.

Case II.—In a house in Fitzwilliam-place, the foot of a metal soil pipe was imperfectly connected with the mouth of an earthenware drain pipe in such a manner that a portion of every discharge passed beside the drain into the foundations. In the servants' bedroom one of the beds was against the wall, just inside the spot where the drain was defective. The servant occupying this bed died of enteric fever.

Case III.—A detached house in Bray had a badly jointed pipe-drain; the connections for the branches were made of short built lengths of brickwork. One of these ran up to the outside of the wall of the house, which below the ground level was constructed of loose open rubble masonry. Inside this wall was the room in which the children slept; two of them were attacked with enteric fever.

Case IV.—In a house in Herbert-place there was an old rubble drain running under the house and delivering into a sewer at the rere; there were some openings between the flags on the kitchen floor, the draught through which, and from the sewer, was strong enough to blow out a candle. The occupier, who was in the prime of life, had a severe attack of enteric fever.

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Case V.—In a house in Haddington-road there was an old rubble drain. The following is an extract from the Engineer's report:—"The smoke test having been applied, and the drain filled with smoke, it was found to be leaking all along its entire length, particularly under the stairs, in the larder, and in the scullery." The occupier had a severe attack of enteric fever.

Case VI.—In a large public building in Dublin the metal trap in the floor of a scullery opening off a large kitchen was defective, the bottom having been broken out. Sewer-gas escaped freely from the broken trap. In an apartment on the same floor, and which had no external window, a servant slept. She contracted enteric fever and died.

Case VII.—In a house in Upper Fitzwilliam-street there was a pipe-drain apparently very carefully jointed. On the smoke test being applied, it was found that a connection existed between the drain and the air space under the boarded floor of a bedroom in the basement, which was occupied by a servant; and the smoke rapidly filled the apartment. The servant, the mistress, and two children suffered from enteric fever.

This case is remarkable, as the pipe work was exceptionally good, the gas only escaping at a junction which received the foot of a waste pipe. There was no intercepting trap on the drain, and therefore sewer-gas from the street sewer had free access to the house.

Case VIII.—In a house in Kingstown the drainage was very defective, 6-inch pipes being thrust into 9-inch without any jointing. There was a lead-lined cistern in the roof over the return; this cistern was connected by a $\frac{3}{4}$-inch lead pipe with that in the next house. The object of this arrangement was to keep the water in both cisterns at the same level, and to allow one overflow pipe to do for both. The second house had an overflow pipe which was trapped and opened into the soil pipe, but the trap was dry, and the sewer-gas had unimpeded access. Originally the connecting pipe between the cisterns had been under the water level (which was regulated by a ball-cock), but the level having been permanently lowered it opened above it, and admitted sewer-gas to the house in question. In this house a married lady died of puerperal fever.

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Case IX.—In a house in Leeson-park the drain was very defective, the sockets being laid down-hill, some lengths of pipe being back to back, and the fall in front being reversed, the drain running up-hill. There was an accumulation of filth under the scullery trough, which was in direct and single-trapped connection with the drain; the trap was very defective. A young married lady living in the house suffered from a severe attack of illness after confinement.

Case X.—In a house in Ely-place the Corporation main sewer ran along the flank wall of the house, the same wall doing duty for house and sewer, the sewer turned in under the larder of the house. There was a bedroom inside the same wall, which was occupied by a servant, who suffered from general debility of an extreme type.

Case XI.—A house in St. Mary's-road was occupied by a lady and gentleman, and several children. All were very fond of baths, and the house was very clean and well kept. The bath-room and closet were combined, and only lighted by a sky-light. The soil-pipe received the rain water from the eaves, which were 3 feet under the level of the sky-light. A so-called ventilator opened on the same level as the sky-light, but owing to the free opening at the gutter the ventilator could hardly be an active agent for either good or ill. In certain winds the sewer-gas escaping from the soil-pipe was blown in through the sky-light, and even when there was no wind the presence of the hot-water cylinder and the frequent use of hot baths combined to induce a steady in-draught of sewer-gas. The bath waste was properly disconnected. A girl, aged thirteen, who used the bath-room regularly, often complained of the smell, and at last contracted diphtheria and died. Three other children also took the disease and died.

Another curious sanitary error was that an air pipe had been taken from the wrong side (*i.e.*, the house side) of the closet trap to the external air, and each time the closet was used there the in-draught of air was so forcible that the lady of the house ascribed a severe attack of piles to it.

That these are no isolated cases is shown by the following table, which has been compiled for me by Mr. Kaye-Parry. It was drawn up after a careful analysis of all the inspections made for the Sanitary Protection Department of the Dublin Sanitary Asso-

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ciation, and shows the percentage of cases in which various sanitary faults were discovered:—

Faults.	Percentage of cases in which they occurred.
Rubble drains under house - - -	26 per cent.
„ „ outside „ - - -	40 „
Total of drains under house - - -	67 „
Defective pipe drains - - - -	61 „
Drains not intercepted from sewer - -	65 „
Unventilated drains - - - -	75 „
W.C.s in centre of house - - -	13 „
„ objectionable - - - -	83 „
Soil pipes unventilated - - -	33 „
„ ventilated inadequately - -	23 „
„ „ dangerously - -	21 „
„ leaking - - - -	44 „
„ running inside house - -	31 „
„ receiving waste pipes - -	45 „
„ badly connected with drain -	32 „
Rain water pipe ventilating sewer unsafely -	46 „
Waste pipes not disconnected - - -	64 „
„ in direct untrapped connection -	25 „
Cisterns overflowing into soil pipes - -	8 „
„ „ drains - -	18 „
Scullery not disconnected - - -	72 „
„ dangerously trapped - - -	50 „
„ untrapped - - - -	9 „
Dry closet defective - - - -	11 „
Surface water untrapped - - -	18 „

It must be remembered that the great majority of these inspections were of houses occupied by members of the Dublin Sanitary Association, so that if a table similar to the above, but analysing all the houses in our city, could be drawn up the percentages would probably be higher. However, even as they are, they show a condition of things that sanitarians, and those interested in preventive medicine, must deplore.

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When faults are suspected it is very important—and especially so in houses in which there is illness—to have them intelligently sought for with a minimum disturbance of the drains. This the Association provides for by having the inspections carried out under the personal supervision of the Engineer, who will also, if required, inspect the carrying out of his recommendations. Both of these points are of great importance, as unskilled meddling with the drains will not only not remove, but may even increase, the faults, and the most perfectly designed system of drainage laid down without close supervision may easily be rendered faulty by the ignorance or carelessness of workmen. Instances illustrating these points are but too commonly met with. In one case where an entirely new system of drainage had been laid down at great expense, everything else was done properly, but the Buchan trap was set the wrong way, the result being that the house was cut off from the ventilator, and the sewer-gas from the street discharged freely in the area.

Another, and very amusing, instance was lately related to me by a medical man who, when sent for to a case which he recognised as enteric fever, at once sought for the source of infection. After a short search he found an iron pipe which opened close to the bedroom window. This he determined must be a ventilating shaft from the drain, and, pronouncing it dangerous, he procured assistance and had it hammered up to prevent any further escape of sewer-gas. The result was not happy. After a time strange sounds were heard, which, when traced to the bath-room, proved to be caused by steam escaping at a high pressure. It was discovered that they had hammered up a safety pipe connected with the heating apparatus. My friend informed me that in future he would prefer to allow an engineer to search for the faults.

The conclusions I would wish to draw are:—

That sewer-gas finds its way into the majority of Dublin houses, and is a fruitful cause of disease.

That its presence ought always to be borne in mind as a possible cause of disease, and that if suspicious symptoms arise, or if the course of an illness is unduly prolonged or irregular, a careful

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examination of the drains, made by a competent person, be at once recommended.

That, in the interests of preventive medicine, householders should be advised to make sure that sewer-gas cannot have access to their houses, this precaution being considered especially necessary before surgical operations, confinements, and removals from house to house.

And if sewer-gas be ever excluded from the houses, I believe it will be found that a great advance has been made towards rendering the expectation of life of our urban population more nearly equal to that of their rural brethren.

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ON THE RESULTS OF THE WORKING OF THE ACTS RELATING TO ADULTERATION IN DUBLIN.

By SIR CHARLES A. CAMERON, M.D.;
President of the Royal College of Surgeons.

[Read in the Sub-Section of State Medicine, April 16, 1885.]

In 1860 the first general Act relating to adulteration was passed. It came into operation on the 6th August, and was entitled "An Act for Preventing the Adulteration of Articles of Food and Drink" (23 & 24 Vict., c. 84). Dr. Letheby was shortly afterwards appointed Public Analyst for the city of London, and Dr. Hill was, I think, in 1862 elected Public Analyst for Birmingham. In 1862 I was elected Public Analyst for Dublin, and I believe that no other appointments were made under the provisions of this Act. The defects in this measure were chiefly as follows:—It did not provide for the appointment of inspectors to obtain articles for analysis; it did not deal with the adulteration of drugs; it vested in the Court of Quarter Sessions the appointment of analysts for districts other than boroughs—a correct procedure so far as England and Wales were concerned, but an incorrect one so far as it related to Ireland, where the Courts of Quarter Sessions have no fiscal business to transact and are merely tribunals. The purchaser of an article which was to be submitted for analysis was obliged to notify the circumstance to the vendor, in order that the latter might have the opportunity of accompanying the purchaser to the analyst. This provision in the Act was an inconvenient one, and rendered the appointment of analysts for remote country districts useless.

So far as I can learn—having made due inquiry—the Act of 1860 proved to be a dead letter everywhere save in Dublin. It is certain that in no other place was there any conviction obtained

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under its provision. In Dublin there are two officials termed deputy clerks of the markets. At my suggestion in 1863 those officers were directed by the Corporation to purchase articles for analysis, not in their capacity as officers, but as citizens. They purchased milk, tea, sugar, flour, bread, coffee, mustard, and other articles, which were analysed, and a large proportion of the specimens found to be adulterated. The milk was especially found to be largely sophisticated. In this year I adopted for milk a standard of 12 per cent. of solids, but I never advised a prosecution unless the solids were below 11·5 per cent. In stating the amount of adulteration I have always expressed it as so much per cent. added to the pure milk. For example, a gallon of water is added to a gallon of milk. It may in such case be said either that the mixture is milk containing 50 per cent. of added water or that it is milk to which 100 per cent. of its weight of water had been added as an adulterant. I invariably employ the latter method, which is now well understood by the Dublin police magistrates. It conveys a better idea of the extent of the adulteration than the other method. To say that milk contains 66 per cent. of added water does not convey clearly to most minds that two gallons of water have been added to a gallon of milk. More than 100 convictions were obtained in Dublin under the operations of the Act of 1860. They referred in great part to adulterated milk; but the vendors of various other adulterated articles were fined.

In 1872 (10th August) an Act was passed to amend the law for the prevention of the adulteration of food, drink, and drugs (35 & 36 Vict., c. 74). It dealt with the adulteration of drugs; it increased the penalty from £5 to £20; it enabled the local authorities to appoint food inspectors; it evidently meant to abolish the necessity for giving notice of intention to have articles analysed, but left the point in a state of ambiguity, the Irish Court of Queen's Bench being equally divided for and against the necessity of notice. It also transferred the power of appointing public analysts in Ireland from the Courts of Quarter Sessions to the Grand Juries, who in Ireland are fiscal.

Immediately after the passing of this Act a food inspector was

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appointed by the Corporation, and in the following year a second inspector was appointed.

On the 27th April, 1874, the House of Commons appointed a Committee to inquire into the operations of the laws relating to adulteration. Three days later the Committee commenced to assemble; they held 14 meetings, and examined 58 witnesses, of whom nine only were chemists, namely—Messrs. Hassall, Voelcker, Tidy, Macadam, Wanklyn, Allen, Bartlett, Sutton, and Cameron. The great majority of the witnesses were persons engaged in the sale of articles liable to adulteration. The evidence of the chemists showed that the Act of 1872 had been productive of good, whilst the testimony of the dealers was chiefly to the effect that it had inflicted injury upon many honest traders, chiefly from want of a proper understanding as to what *does and what does not* constitute adulteration, and also from the inexperience of the majority of the analysts. The Committee recommended certain changes in the Act, some of them in the interest of the vendor; and upon the lines recommended by the Committee the present Act, called "The Sale of Food and Drugs Act," was laid down (38 & 39 Vict., c. 63, 1875). It enabled the purchasers of articles intended for analysis to send them by registered letter—a change which enabled many of the Grand Juries to appoint chemists in distant towns to act as analysts for counties. It did not provide for the publication of the name, address, and offence of persons convicted a second time, a clause to that effect having existed in the two previous Acts. This omission is to be regretted. In Dublin the conviction of "old offenders" was on many occasions published at their own expense in the advertising part of the newspapers, and placarded on the walls close to their shops. I cannot understand why this provision was omitted from the Act of 1875.

In my evidence before the Committee I insisted upon the necessity of empowering the inspectors to purchase articles compulsorily for analysis. The present Act imposes a penalty of £10 on persons who refuse to sell the articles asked for by the inspectors; the penalty ought to be £20, the maximum penalty for selling an adulterated article.

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The Sale of Food and Drugs Act has been carried out pretty fully in Dublin, as, indeed, were the two earlier Acts, so far as it was practicable. When they were first put in operation there was a pretty large amount of adulteration practised, as in other towns. Coffee and mustard were rarely met with in a pure state. The former, as a rule, contained chicory, burnt sugar, roasted grain, and other impurities; the latter always was more or less composed of wheat or rice flour, and, in a few instances, of sulphate of calcium. Tea was occasionally, but not frequently, adulterated, chiefly with exhausted tea-leaves. Sugar was, except in two instances, always found to be pure. Bread was found to be adulterated with rice-flour, potatoes, and alum; and flour with rice and alum.

In 1870 I made an examination of 123 specimens of confections. Those manufactured at three establishments were quite pure. The collections obtained at ten other shops contained poisonous pigments and other impurities in a large proportion of the items. Out of 40 confections coloured yellow only 2 owed their colour to saffron. One was coloured with gamboge, and all the others were coated with chromate of lead, in amounts varying from $\frac{1}{400}$ to less than $\frac{1}{1000}$ of the weight of the confections. The common sugarstick, sold at 1½d. per two ounces, contained the largest proportion of chromate of lead. Twelve articles, chiefly lozenges and "sugar almonds," had a bright orange hue, due to the presence of a variety of chromate of lead. Thirty-eight of the specimens—comprising "peaches," sugar almonds, lozenges, comfits, sugarstick, sugar balls, &c.—had various shades of red, from a faint pink to a bright scarlet. Of these, 36 specimens were coloured with cochineal, while 2 contained vermilion in the proportion of 4 grains per ounce of the confection, which was the cheapest kind of sugarstick.

At one time arsenite of copper was frequently used to impart a brilliant green colour to confectionery, but the numerous accidents that occurred from the employment of this poisonous pigment have so alarmed the public that green confectionery is rarely met with.[a]

[a] Turnip leaf pulp is sometimes used in Dublin to produce green confections.

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Only one of the specimens above referred to was a small figure of a baby with a green frock on it. The colour was composed of a mixture of Prussian blue and chromate of lead. Blue is not a popular colour; only one specimen having streaks of this colour was contained in 123 samples. The pigment employed was ultramarine. Nine specimens were brilliantly coloured with mauve, magenta, &c. As the coal-tar dyes are liable to contain traces of lead, mercury, and arsenic, and as their use for the purpose of colouring confectionery has been prohibited by the authorities in Paris, it would appear to be the safer plan not to place these dyes on any substance intended for human food. The quantity used, however, is so small, that no serious consequences are likely to arise from eating confections coloured with these substances.

An article of food containing more than a grain of chromate of lead per ounce may well be regarded as a slow poison. Soft water containing less than ·1 grain of lead per gallon has often produced poisonous effects on individuals and families. Chromium is also a poisonous metal. Six grains of a salt of this metal injected into the jugular vein of a dog caused the death of the animal. Workmen engaged in the preparation of chromate of potash often suffer from an ulceration of the throat resembling that of secondary syphilis, and also from slow necrosis of the nasal bones. I have not the slightest doubt but that the use of confectionery coloured with chromate of lead produces a large amount of infantile disease.

Vermilion was at one time an officinal medicament, but it is no longer administered internally, though still employed in mercurial fumigations. It constitutes the basis of Boerhaave's red pill—a celebrated nostrum of former days. There is a general belief in the inertness of this salt of mercury; but I find that it is capable of producing mercurialism, when given in very large doses. In 1½d. worth (2 ozs.) of sugarstick I found 8 grains of this salt. As the dose of calomel—one of the mildest compounds of mercury—for a young child is only a grain, it is evident that vermilion, notwithstanding its great insolubility and comparative inertness, if consumed by a child at the rate of even 2 or 3 grains per diem, would produce some ill result. From experiments which

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I made at the Dublin Lock Hospital, I found that patients put upon large doses of vermilion became after a time mercurialised. Vermilion is liable to be adulterated. The Chinese vermilion is generally very pure, but cheaper varieties may contain minium. In the vermilion used to colour the confectionery which came into my possession, I found nearly 20 per cent. of red lead—a far more active poison than mercuric sulphide. Lozenges purchased at two of the 13 sugar confectioners' shops contained from 12 to 40 per cent. of an insoluble white clay, known in the trade under the term of *terra alba*. The peaches, sugar almonds, lozenges, and comfits contained rice-starch and gum arabic. Cough lozenges and Bath pipe contained gum, sugar, and extract of liquorice, a few of them being slightly medicated by the addition of opium and camphor (probably in the form of paregoric elixir). Small quantities (under 3 per cent.), of plaster of Paris were found in the Bath pipe and cough lozenges, but they were probably derived from adulterated liquorice extract.

A figure of a baby in its cradle had the following composition :— The cradle was composed of a mixture of plaster of Paris (calcic sulphate) and sugar; the body of the baby was sugar and rice-starch; its eyes were Prussian blue; its cheeks were tinted with cochineal; and its dress was painted with chromate of lead.

The ten sugar confectioners above-mentioned, whose wares I have now described, were prosecuted before the police magistrates. Five of them, on paying costs (£3), and promising to abandon the use of poisonous pigments, were "let off with a caution." The others were fined respectively £5, and £3 costs; £1, and £1 costs; £1, and £1 costs; 10s., and £3 costs; and 10s.

A great many forms of adulteration have been exposed in Dublin through the agency of the Acts relating to adulteration. Since the year 1870, nearly 700 persons have been convicted in Dublin for selling adulterated food, and the fines imposed upon them have amounted to nearly £3,200. At present very few articles are liable to adulteration save milk and butter. None of the specimens of coffee, tea, sugar, mustard, pepper, and liquors, lately collected for analysis, have proved to be adulterated.

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I have occasionally taken advantage of the provision of the section of the Dublin Improvement Act of 1849 to reach food adulterators who could not be got at readily under the provisions of the Sale of Food and Drugs Act. The section to which I refer is that which provides a penalty of £1 upon anyone who sells any article of food adulterated or fraudulently made. The simplicity of this section I should be glad to see imitated. It does not provide that the samples to be analysed are to be divided into three parts, and that notice of intention to have one of them analysed is to be given to the vendor. When a person purchases an article with the *bonâ fide* intention of consuming it himself, and subsequently is led to suspect its purity, the Dublin Improvement Act enables him to prosecute the vendor, should his suspicions be verified. The fact of the adulteration and the sale of the article have only to be proved to the satisfaction of the magistrate.

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OBSERVATIONS ON THE RELATIVE PREVALENCE OF DISEASE AND THE RELATIVE DEATH-RATES IN TOWN AND COUNTRY DISTRICTS IN IRELAND.

By THOMAS W. GRIMSHAW, M.A., M.D., Univ. Dubl.;
Registrar-General for Ireland.

[Read in the Sub-Section of State Medicine, April 16, 1885.]

The prevalence of disease and the high death-rates among town populations, as compared with country populations, have always attracted the attention of sanitarians and vital statisticians, and it is admitted by all that under the conditions in which society at present exists the chances of prolonged life are less among the inhabitants of towns than among country people.

It has been my duty within the past four years to give considerable and special attention to the subject of the distribution of disease and deaths in Ireland—firstly, in connexion with the census of 1881, when extensive statistics regarding the prevalence of disease were collected; and secondly, when compiling a decennial summary of the statistics of the births, marriages, and deaths in Ireland for the inter-census period from 1870 to 1881.[a]

Various statistics relating to this subject have been published in the tables issued by my department, and considered in official reports, including that referred to. As, however, these reports contain many other matters, and are arranged for the purpose of illustrating many points unconnected with that now under discussion, I have thought it desirable to extract and bring together from

[a] Supplement to the Seventeenth Report of the Registrar-General of Marriages, Births, and Deaths in Ireland, containing Decennial Summaries for the Years 1871-1880. Presented to Parliament by command of Her Majesty. Dublin: Alexander Thom & Co., Limited. 1884.

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both sets of reports the principal facts bearing upon the relative health of town and country populations in Ireland, which I believe will prove interesting and instructive.

In the decennial summary of marriages, births, and deaths already referred to, I have prepared a table classifying the population of Ireland into town or "civic" populations and rural populations, and grouped the deaths according to these divisions.

Some little difficulty was experienced in determining what should be classed as civic and what as rural populations. After careful consideration of all the circumstances I determined to consider a populous place with municipal government containing a population of 10,000 or upwards as a town, and to term the district in which that town was situated a "civic" district, and the population of the whole district a town or civic population—the area adopted as comprising the civic population being the poor law union or superintendent registrar's district in which such populous place was situated.

It may be objected that a population of 10,000 or upwards residing in a town situated in the midst of a country district—say the case of Sligo, where the inhabitants of the town number 10,808, and of the union 46,063—is not fairly a town population. This, however, is an extreme case. I think it cannot be denied that a busy town materially affects the whole population for many miles round, not only as to its social and economic conditions, but also as to its health conditions. Thus if we take a business town with suburbs it is clear that the persons who conduct their business in that town and have their residence in the adjacent country, have their health affected both by town and country influences, and therefore must be considered from a sanitary point of view as inhabitants of both districts; and as in every case the circumstances of town life appear to be the predominating influence, I have selected a district with a town population of 10,000 or upwards as a fair limit for civic districts.

The result of this classification of the population of Ireland is that the mean civic population in the decade 1871–80 was 1,476,929, and the rural population 3,816,678, making a total mean population

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for the decade of 5,293,607. The deaths among the civic population were 332,526, or at the annual average rate of 22·5 per 1,000; the deaths in the rural population were 634,219, or at the annual average rate of 16·6 per 1,000, or about 5·9 per 1,000 less. Therefore, in round numbers, the deaths among a million of such a civic population would exceed the deaths among a rural population of the same amount by about 6,000 per annum; or if the rural population of Ireland died at the same rate as the civic there would be an average increase of about 22,000 deaths per annum.

To illustrate the influence of town populations in the mortality of the areas in which they are situated, I may quote a statement from the decennial summary, where the registration counties of Ireland are arranged in the order of their rates of mortality, from the lowest to the highest, as follows[a]:—

DEATHS REGISTERED, 1871–80.

Registration Counties	Death-rate per 1,000	Registration Counties	Death-rate per 1,000
1 Mayo	13·9	17 Kildare	17·7
2 Sligo	14·1	18 Meath	17·8
3 Roscommon	14·3	19 Tipperary	18·1
4 Leitrim	14·4	20 Queen's	18·1
5 Galway	15·2	21 Cork	18·6
6 Donegal	15·2	22 Armagh	18·9
7 Clare	15·6	23 Limerick	19·3
8 Fermanagh	15·7	24 Louth	19·3
9 Longford	15·7	25 Westmeath	19·4
10 Cavan	16·1	26 Down	19·4
11 Tyrone	16·4	27 Wexford	19·7
12 Monaghan	16·5	28 Carlow	20·0
13 Kerry	16·6	29 Kilkenny	20·4
14 Wicklow	17·1	30 Antrim	20·7
15 King's	17·4	31 Waterford	22·0
16 Londonderry	17·7	32 Dublin	26·2

A glance at the list and a comparison between it and Tables I. and II. will show that the counties containing large towns occupy the most unfavourable positions on the list.

Now, turning from deaths to statistics of disease as furnished by the census returns of 1881 (Tables III. and IV.), we find that, exclusive of lunatics, blind, deaf and dumb, there were returned as

[a] Seventeenth Report of the Registrar-General of Ireland, p. 17.

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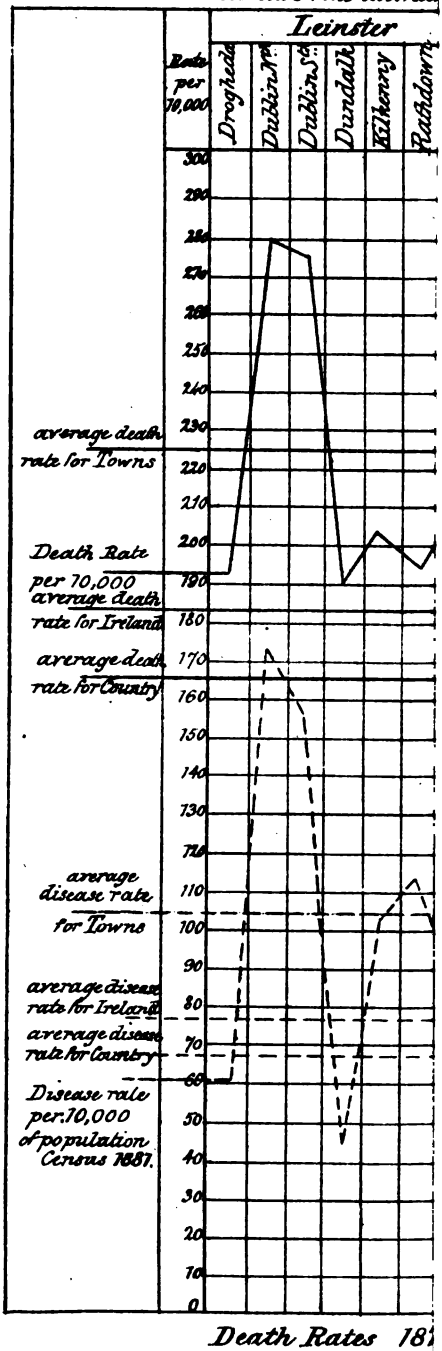

Death Rates 18

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sick ("temporarily sick" is the term used in census report) on census night 40,090 persons, or at the rate of 7·75 per 1,000 of the population of Ireland. Of these 40,090 persons, 15,571 or at the rate of 10·51 per 1,000 of the population, were in civic districts; and 24,519, or at the rate of 6·64 per 1,000 in the rural districts.

The death statistics are much more exact than the statistics of disease. It will be observed that the ratios between the town and country death-rate and the town and country disease-rate are very close to one another, being in the former at the rate of 1·5 to 1 and in the latter 1·4 to 1. This point is of very great interest and of considerable importance, as it shows that, when dealing with the statistics of large communities, the census statistics of sickness may be taken as a fair general measure of the comparative health of sections of the population.

If we extract the figures from the tables (I. to IV.) which I have already alluded to, and construct curves to illustrate the comparison, we have the accompanying diagram (Diagram I.), which shows that the relations pointed out as existing between the average death-rate for Ireland during the decade and the disease-rate on the census night are tolerably constant. There are some exceptions, both absolute and relative to this rule, but these exceptions arise chiefly among the smaller numbers, and in the case of the larger populations the curves are nearly parallel.

I have here introduced this reference to death-rate and sickness-rate with a twofold object—first, to point to the importance of a registration of disease; secondly, to substantiate by the only available record of the prevalence of disease the value of the death-rate as a measure of the health of communities. I have elsewhere indicated the precautions which I consider essential in measuring the health of communities by their death-rates, and it is not necessary to again repeat these cautions here.[a]

It is scarcely necessary again to point out that the death-rates in towns are higher than the death-rate in country districts.

[a] Address to Section I. of the Sanitary Congress in Dublin in 1884, entitled "Statistical Measures of the Health of Communities."

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There are many difficulties in assigning causes why the expectation of life should be less in town than in country.

There are many reasons why town populations should be more healthy, instead of less healthy, than country populations. They should be better housed, better supplied with water, should have more light at night, have better sanitary appliances, better cleansing arrangements, and have, as a matter of fact, better provision for the sick of all classes—good hospitals for the poor, and the best medical advice for the rich. On the other hand, they have the disadvantages of being closely packed together, the air is less pure, they are engaged in unhealthy occupations, and liable to habits and temptations which promote disease. Looking at such a statement as the foregoing, it would almost appear that the advantages of town life should outweigh the disadvantages. Unfortunately the advantages are seldom developed to anything like a reasonable extent, and the disadvantages are, in too many instances, intensified as far as they possibly may be.

Now, we naturally expect that some diseases will prevail and cause deaths to a greater extent in town than in country districts. Thus where people are close together infective diseases will spread with greater facility than where people are widely separated, and, therefore, diseases of the infective type are found to be more prevalent in town than in country populations. But why should this be also true of other forms of disease?

In the accompanying table of causes of death from particular diseases and certain well-marked groups of diseases, it will be observed that, with the exception of childbirth, all the principal causes of death are more intense in town than in country districts in Ireland. The accompanying diagram demonstrates this in a very distinct manner (see Diagram II.).

Looking at Tables I. and II. and Diagram II. it will be observed there are three principal groups of causes of death—namely, the principal zymotics, phthisis or pulmonary consumption, and diseases of the respiratory organs. Leaving other causes out of consideration we have the following general statement:—

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Causes of Death.
Smallpox
Measles
Scarlatina
Diphtheria
Whooping Cough

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	Total Deaths		Principal Zymotics		Phthisis		Diseases of Respiratory Organs	
	No.	Rate per 1,000	No.	Rate per 1,000	No.	Rate per 1,000	No.	Rate per 1,000
Civic Districts	332,526	22·5	47,656	3·2	40,840	2·8	56,724	3·8
Rural Districts	634,219	16·6	67,391	1·8	62,688	1·6	86,267	2·3
Total Ireland	966,745	18·3	115,047	2·2	103,528	2·0	142,991	2·7

It will be observed that these three groups comprise more than one-third of the mortality from all causes in Ireland, and that in each group the death-rate in towns far exceeds that of the average for Ireland, and of course still further that of the country districts. A further analysis shows that in the case of the principal zymotics the death-rate in towns exceeds that of the average of Ireland from the same causes by 10·6 per 10,000; in the case of phthisis, by 8·1; and in cases of diseases of the respiratory organs, 11·4. The excess of mortality similarly taken in civic as compared with rural populations is—principal zymotics, 14·5; phthisis, 11·3; and diseases of the respiratory organs, 15·8. It is evident from the foregoing statement that the greatest waste of life which takes place in town as compared with country populations in Ireland is caused by diseases of the respiratory organs and phthisis, not by the principal zymotic diseases of an infective nature. In dealing with this question it is difficult to treat of phthisis and diseases of the respiratory organs separately, as they are, in fact, often combined in causing death, and many cases of phthisis originate in some other form of lung disease. If we take these two groups in combination we find that during the decade 1871–80 they caused 246,519 deaths, or more than one-fourth of all the deaths in Ireland.

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TABLE I.—*Average Annual Rates per* 10,000 *of the Mean Population* (1871–81) *represented by the Deaths from Several Causes registered during the Ten Years*, 1871–80, *in* (1) *the Unions or Superintendent Registrars' Districts containing Towns which in* 1871 *or in* 1881 *had a population of* 10,000 *or upwards;* (2) *the Group of such Unions in each Registration Province;* (3) *the remaining portion of each Province;* (4) *the total of each Province; with corresponding Rates for all Ireland.*

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881) situated in the respective Unions	Mean Population of Superintendent Registrars' Districts, 1871–81	Average Annual Rate (1871–80) per 10,000 of the Mean Population represented by Deaths from																	
			All Causes	Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping-cough	Fever	Erysipelas	Metria, or Puerperal Fever	Childbirth	Influenza	Dysentery	Diarrhœa	Cholera	Principal Zymotic Diseases	Phthisis or Consumption	Diseases of Respiratory Organs	Violence	Other Causes and Unspecified Causes
PROVINCE OF LEINSTER.																				
Drogheda, -	Drogheda, - -	33,178	193·9	1·4	1·8	7·2	1·8	2·5	5·1	·6	·5	·9	·1	·4	3·1	·1	23·4	24·7	25·3	4·3	114·1
Dublin North, } Dublin South. }	Dublin, Rathmines and Rathgar, and Pembroke,	138,542 / 200,888	279·0 / 274·3	8·1 / 10·2	5·6 / 5·0	10·1 / 9·1	·9 / 1·0	5·5 / 5·4	9·9 / 9·4	1·2 / 1·0	1·8 / ·8	1·4 / 1·1	·1 / ·4	·6 / ·4	7·9 / 6·7	·4 / ·2	49·0 / 47·4	33·4 / 30·0	52·9 / 53·3	7·0 / 6·6	132·2 / 133·7
Dundalk, -	Dundalk, - -	44,297	190·5	·7	1·9	4·3	·6	4·2	4·3	·8	·6	1·2	·2	·5	3·9	·2	20·6	15·9	31·6	5·2	114·4
Kilkenny, -	Kilkenny, - -	29,963	204·4	—	2·4	4·3	·4	1·5	6·8	·7	·4	1·3	·1	·4	5·2	·2	21·2	19·1	31·6	3·8	126·2
Rathdown, -	Kingstown, - -	56,305	195·2	2·4	2·7	6·7	1·1	2·7	6·7	·9	·4	·7	·1	·3	3·5	·4	26·5	23·8	32·3	4·4	106·1
Wexford, -	Wexford, - -	39,692	210·1	·9	·9	3·7	·9	4·3	4·2	·7	·4	·7	·0	·4	2·7	·1	18·1	25·9	29·1	4·4	130·8
Total Civic Unions,* - -		542,865	247·0	6·3	4·0	7·9	1·0	4·6	8·0	1·0	1·0	1·1	·2	·4	5·9	·3	38·4	27·8	44·6	5·9	127·0
Rural Unions,† - -		742,870	182·4	·6	1·6	3·5	·8	3·0	5·0	·5	·5	1·1	·2	·4	2·6	·1	17·6	20·4	23·6	3·7	114·8
Total of Province, -		1,285,735	209·7	3·0	2·6	5·4	·9	3·7	6·3	·7	·7	1·1	·2	·4	4·0	·2	26·5	23·6	32·4	4·6	119·9
PROVINCE OF MUNSTER.																				
Clonmel, -	Clonmel, - -	23,621	230·9	·5	1·0	6·3	1·1	4·2	5·3	·5	5	1·0	·0	·5	6·6	·4	25·9	20·3	48·1	4·3	130·3
Cork, - -	Cork and Queenstown	144,298	214·8	6·8	4·2	6·6	·6	4·1	7·5	·7	·6	1·0	·0	·5	5·3	·2	35·8	27·4	38·7	6·2	124·2
Limerick, -	Limerick, - -	77,149	208·7	·1	1·7	5·0	·6	3·6	6·3	·4	·5	1·0	·0	·9	6·8	·2	25·2	20·7	33·5	4·6	122·8
Waterford, -	Waterford, - -	56,248	233·9	·2	3·8	6·7	·2	3·2	6·0	·7	·9	·8	·1	·7	6·6	·1	27·5	27·7	31·2	5·6	139·4
Total Civic Unions,* - -		301,316	227·6	3·3	3·2	6·2	·6	3·8	6·7	·6	·8	1·0	·0	·6	6·0	·2	30·6	25·2	36·7	5·5	127·2
Rural Unions,† - -		1,075,321	171·3	·4	2·6	3·4	·4	3·4	6·1	·6	·7	1·1	·2	·7	3·6	·2	20·8	13·6	26·7	3·6	104·0
Total of Province, -		1,376,637	183·6	1·0	2·7	4·0	·4	3·5	6·2	·6	·7	1·1	·2	·7	4·2	·2	22·9	16·1	29·0	4·0	109·0

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Province of Ulster.																				
Armagh, - -	Armagh,	68,120	191·2	·2	1·1	2·5	·8	3·3	5·6	·6	·7	·8	·4	·3	3·0	·3	17·1	24·2	31·4	3·8	112·2
Belfast, - -	Belfast, - -	220,962	241·0	3·1	5·3	7·7	·9	7·0	9·1	·8	·8	·8	·1	·4	8·5	·3	42·3	38·2	49·9	5·2	103·1
Lisburn, - -	Lisburn, - -	57,666	200·6	1·3	1·8	6·0	·8	4·0	5·3	·5	·5	1·3	·2	·3	5·1	·1	24·7	29·9	29·7	3·6	110·2
Londonderry, -	Londonderry,	59,184	186·3	2·5	·9	7·4	1·0	4·0	4·9	·7	·4	·9	1	·2	2·4	·1	23·4	24·0	20·1	4·4	112·3
Lurgan, - -	Lurgan, - -	66,449	190·8	·4	1·5	6·1	·7	2·6	5·2	·6	·8	·9	·2	·3	5·4	·1	22·3	23·5	26·9	3·1	112·5
Newry, - -	Newry, - -	69,900	185·8	·6	1·1	3·0	·7	3·0	4·9	·3	·5	·8	·4	·6	2·6	·1	16·6	24·3	25·5	4·3	113·1
Total Civic Unions,* -		542,281	211·3	1·8	2·9	6·0	·8	4·9	6·8	·5	·7	·9	·2	·4	5·7	·2	29·5	30·4	36·2	4·4	108·5
Rural Unions,† -		1,260,219	166·8	·3	1·0	4·1	·8	2·9	4·3	·5	·6	1·0	·2	·3	2·4	·1	16·3	19·3	21·7	3·3	104·0
Total of Province, -		1,802,500	180·2	·7	1·6	4·8	·8	3·5	5·0	·5	·6	1·0	·2	·3	3·4	·1	20·2	22·7	26·1	3·6	105·3
Province of Connaught.																				
Galway, - -	Galway, - -	43,561	186·9	2·6	·9	1·4	·4	3·4	8·0	·6	·6	1·1	·0	·6	1·9	0	19·2	16·8	19·3	6·1	123·2
Sligo, - -	Sligo, - - -	46,906	152·2	·1	·7	3·4	·4	2·3	4·6	·4	·4	·9	·1	·1	2·8	·0	14·4	19·0	21·2	3·7	92·1
Total Civic Unions,* -		90,467	168·9	1·3	·8	2·5	·4	2·8	6·2	·5	·5	1·0	·1	·3	2·3	·0	16·6	18·0	20·3	4·8	107·1
Rural Unions,† -		738,268	141·3	1·0	1·5	2·7	·3	3·2	5·2	·3	·6	1·3	·2	·4	1·6	·0	15·9	11·6	17·2	2·7	91·5
Total of Province, -		828,735	144·3	1·0	1·4	2·6	·3	3·2	5·3	·4	·6	1·3	·2	·4	1·7	·0	15·9	12·3	17·5	3·0	93·1
Ireland.																				
Total Civic Unions,* -		1,476,929	225·1	3·8	3·2	6·5	·8	4·4	7·2	·7	·8	1·0	·1	·5	5·6	·2	32·2	27·7	38·4	5·2	119·0
Total Rural Unions,† -		3,816,678	166·2	·5	1·7	3·5	·6	3·1	5·1	·5	·6	1·1	·2	·5	2·6	·1	17·7	16·4	22·6	3·4	103·7
Total, - - -		5,293,607	182·6	1·4	2·1	4·3	·6	3·5	5·7	·6	·7	1·1	·2	·5	3·4	·1	21·6	19·6	27·0	3·9	107·9

* Unions or Superintendent Registrars' Districts containing Towns which in 1871 or in 1881 had a population of 10,000 or upwards.
† All Unions other than those specified in Note (*).

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TABLE II.—*Total Deaths and Deaths from several causes registered during the Ten Years,* 1871–80, *in those Unions or Superintendent Registrars' Districts containing Towns which in* 1871 *or in* 1881 *had a population of* 10,000 *or upwards; with a Summary thereof for each Registration Province, and corresponding details for the remaining portion of each Province.*

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881) situated in the respective Unions	Mean Population of Superintendent Registrars' Districts, 1871–81	Total Deaths	Deaths from several Causes in the Ten Years, 1871–80																	All other Causes and Unspecified Causes
				Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping-cough	Fever	Erysipelas	Metria or Puerperal Fever	Childbirth	Influenza	Dysentery	Diarrhœa	Cholera	Principal Zymotic Diseases	Phthisis or Consumption	Diseases of Respiratory Organs	Violence	
PROVINCE OF LEINSTER.																					
Drogheda, -	Drogheda, - -	33,178	6,434	45	60	239	60	84	169	20	15	29	3	13	103	4	777	821	841	142	3,786
Dublin North, } Dublin South. }	Dublin, Rathmines and Rathgar, and Pembroke,	133,542 / 200,888	38,649 / 55,141	1,128 / 2,043	767 / 996	1,396 / 1,826	130 / 209	764 / 1,807	1,364 / 1,892	160 / 212	252 / 171	200 / 220	8 / 82	82 / 78	1,094 / 1,247	57 / 41	6,782 / 9,519	4,629 / 6,026	7,332 / 10,707	969 / 1,329	18,317 / 26,875
Dundalk, -	Dundalk, - -	44,297	8,439	31	84	191	25	185	192	34	29	54	7	23	170	10	911	706	1,401	229	5,068
Kilkenny, -	Kilkenny, - -	29,963	6,134	—	73	129	11	46	203	22	13	39	2	12	157	6	637	574	948	114	3,785
Rathdown, -	Kingstown, - -	56,305	10,932	136	153	375	62	151	378	48	25	39	3	16	195	22	1,488	1,333	1,809	215	5,942
Wexford, -	Wexford, - -	39,692	8,340	38	37	146	38	171	166	28	14	26	1	15	109	3	723	1,028	1,154	173	5,193
Total Civic Unions,* - -		542,865	134,069	3,421	2,170	4,302	535	2,488	4,364	524	519	607	106	239	3,175	143	20,837	15,117	24,192	3,201	68,966
Rural Unions,† - -		742,870	135,528	489	1,173	2,615	562	2,232	3,706	410	402	822	145	276	1,896	72	13,020	15,191	17,532	2,740	85,265
Total of Province, -		1,285,735	269,597	3,910	3,343	6,917	1,097	4,720	8,070	934	921	1,429	251	515	5,071	215	33,858	30,308	41,724	5,941	154,231
PROVINCE OF MUNSTER.																					
Clonmel, -	Clonmel, - -	23,621	5,448	11	24	149	25	100	125	13	11	23	1	12	155	10	611	480	1,134	101	3,074
Cork, - -	Cork and Queenstown	144,298	33,883	978	604	959	96	593	1,077	93	129	142	4	69	760	28	5,164	3,955	5,584	894	17,918
Limerick, -	Limerick, - -	77,149	16,103	7	128	384	43	278	487	32	37	79	2	72	522	17	1,938	1,601	2,586	357	9,471
Waterford, -	Waterford, - -	56,248	13,158	10	213	379	12	182	336	38	49	47	3	41	374	6	1,553	1,557	1,753	315	7,843
Total Civic Unions,* - -		301,316	68,592	1,006	969	1,871	176	1,153	2,025	176	226	291	10	194	1,811	61	9,266	7,593	11,057	1,667	38,306
Rural Unions,† - -		1,075,321	184,150	449	2,793	3,642	414	3,664	6,512	597	787	1,219	199	734	3,925	224	22,357	14,555	28,668	3,887	111,881
Total of Province, -		1,376,637	252,742	1,455	3,762	5,513	590	4,817	8,537	773	1,013	1,510	209	928	5,736	285	31,623	22,148	39,725	5,554	150,187

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Province of Ulster.																					
Armagh, - -	Armagh, - -	68,120	13,001	13	77	172	54	227	378	39	45	58	27	20	207	23	1,171	1,644	2,132	252	7,663
Belfast, - -	Belfast, - -	220,962	53,294	694	1,161	1,700	198	1,557	2,014	140	172	179	14	90	1,884	76	9,374	8,444	11,028	1,143	22,790
Lisburn, - -	Lisburn, - -	57,666	11,633	76	103	393	41	232	309	30	29	75	10	20	296	6	1,476	1,737	1,724	209	6,343
Londonderry, -	Londonderry, -	59,184	10,991	149	51	437	63	236	290	39	29	52	8	12	141	6	1,385	1,413	1,183	259	6,623
Lurgan, - -	Lurgan, - -	66,449	12,687	28	98	407	44	185	348	37	51	57	12	18	359	7	1,494	1,562	1,786	208	7,480
Newry, - -	Newry, - -	69,900	12,987	40	77	206	50	209	343	22	34	55	25	38	179	7	1,149	1,702	1,783	299	7,918
Total Civic Unions,* -		542,281	114,583	1,000	1,567	3,315	450	2,646	3,682	307	360	476	96	198	3,066	125	16,049	16,502	19,636	2,370	58,787
Rural Unions,† -		1,260,219	210,241	355	1,284	5,127	1,042	3,610	5,431	637	728	1,370	281	412	2,993	71	20,325	24,374	27,385	4,206	131,045
Total of Province, -		1,802,500	324,824	1,355	2,851	8,442	1,492	6,256	9,113	934	1,088	1,746	377	610	6,059	196	36,374	40,876	47,021	6,576	189,832
Province of Connaught.																					
Galway, - -	Galway, - -	43,561	8,143	114	40	62	16	149	347	26	24	48	1	26	81	1	836	731	843	267	5,367
Sligo, - -	Sligo, - - -	46,906	7,139	3	31	161	19	105	214	18	20	43	5	5	128	2	668	897	996	173	4,319
Total Civic Unions,* -		90,467	15,282	117	71	223	35	254	561	44	44	91	6	31	209	3	1,504	1,628	1,839	440	9,686
Rural Unions,† -		738,268	104,300	713	1,091	1,982	189	2,387	3,811	255	478	980	129	303	1,178	34	11,688	8,568	12,682	2,027	67,493
Total of Province, -		828,735	119,582	830	1,162	2,205	224	2,641	4,372	299	522	1,071	135	334	1,387	37	13,192	10,196	14,521	2,467	77,179
Ireland.																					
Total Civic Unions,* -		1,476,929	332,526	5,544	4,777	9,711	1,196	6,541	10,632	1,051	1,149	1,465	218	662	8,261	332	47,656	40,840	56,724	7,678	175,745
Total Rural Unions,† -		3,816,678	634,219	2,006	6,341	13,366	2,207	11,893	19,460	1,889	2,395	4,291	754	1,725	9,992	401	67,391	62,688	86,267	12,860	395,684
Total, - - -		5,293,607	966,745	7,550	11,118	23,077	3,403	18,434	30,092	2,940	3,544	5,756	972	2,387	18,253	733	115,047	103,528	142,991	20,538	571,429

* Unions or Superintendent Registrars' Districts containing Towns which in 1871 or in 1881 had a population of 10,000 or upwards

† All Unions other than those specified in Note (*).

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TABLE III.—*Showing the Rate per* 1,000 *of the Population of Persons* (1) *those Unions or Superintendent Registrars' Districts containing Unions in each Registration Province;* (3) *the remaining portion of for all Ireland.*

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881), situated in the respective Unions	Area in Statute Acres	Population in 1881	Number per 1,000 of Population Temporarily Diseased				Zymotic				
				At their own Homes	In Infirmaries and General and Special Hospitals	In Workhouse Hospitals	Total	Smallpox	Measles	Scarlatina	Diphtheria	Whooping-cough
PROVINCE OF LEINSTER.												
Drogheda, -	Drogheda, - -	99,062	31,630	3·51	—	2·50	6·01	·03	—	·19	—	—
Dublin North, } Dublin South, }	Dublin, Rathmines and Rathgar, and Pembroke.	41,256 / 43,200	142,981 / 202,264	1·35 / 2·02	4·64 / 4·94	11·40 / 8·67	17·39 / 15·63	— / ·02	·03 / ·03	·06 / ·16	·01 / ·00	— / ·00
Dundalk, -	Dundalk, - -	104,629	43,193	3·15	·38	·44	4·47	—	—	—	—	—
Kilkenny, -	Kilkenny, - -	110,943	28,733	3·83	1·53	4·80	10·16	—	·07	·07	—	—
Rathdown -	Kingstown, - -	61,513	57,913	2·59	1·31	7·05	11·45	·14	—	·05	—	·03
Wexford, -	Wexford, - -	126,901	38,745	4·21	·67	3·61	8·49	—	—	·08	—	—
Total Civic Unions,* - -		531,904	545,459	2·33	3·44	7·64	13·41	·02	·02	·10	·00	·01
Rural Unions,† - -		4,114,598	710,562	3·77	·57	3·13	7·47	·01	·08	·07	—	·03
Total of Province, -		4,706,502	1,256,021	3·15	1·81	5·09	10·05	·02	·06	·08	·00	01
PROVINCE OF MUNSTER.												
Clonmel, -	Clonmel, - -	87,707	23,009	4·35	·39	5·30	10·04	—	—	·39	—	·04
Cork, - -	Cork and Queenstown.	169,732	145,216	3·12	3·13	7·92	14·17	·01	·09	·08	—	·08
Limerick, -	Limerick, - -	177,956	75,051	2·29	1·20	7·90	11·39	—	—	·05	—	—
Waterford, -	Waterford, - -	125,720	55,189	4·44	·96	5·24	10·64	—	·05	·15	—	—
Total Civic Unions,* - -		561,115	298,465	3·25	2·08	7·22	12·50	·00	·05	·11	—	·03
Rural Unions,† - -		5,444,559	1,046,544	3·14	·28	3·84	7·26	·00	·06	·08	·00	·05
Total of Province, -		6,005,674	1,345,009	3·16	·67	4·59	8·42	00	·06	·09	·00	·03

* Unions or Superintendent Registrars' Districts containing

† All Unions other than those specified in Note (*).

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who laboured under Temporary Diseases on the 3rd of April, 1881, in Towns with a Population of 10,000 or upwards; (2) the Group of such each Province; (4) the total of each Province; with corresponding Rates

Nature of Illness and Rate per 1,000																						
Diseases						Constitutional Diseases		Local Diseases of								Developmental Diseases			Injuries	Unspecified or Ill-defined
Fever	Diarrhœa, Dysentery	Cholera	Principal Zymotic Diseases	Rheumatism	Other Zymotics	Consumption	Others	Nervous System	Circulatory Organs	Respiratory Organs	Digestive Organs	Urinary Organs	Generative Organs	Organs of Locomotion	Integumentary System	Childbirth	Debility, Old Age	Others		
·13	·06	—	·41	·48	·09	·63	·26	·38	·22	1·43	·13	·09	—	·09	·16	·06	·82	—	·28	·48
·41	·06	—	·57	1·33	·92	·64	·38	1·32	·38	2·15	·40	·18	·20	·39	1·13	·24	5·80	—	·66	·70
·46	·04	—	·73	1·06	1·04	·50	·69	1·59	·98	1·60	·29	·17	·22	·54	·78	·16	4·57	·01	·62	·81
·07	·05	—	·12	·44	·30	·49	·28	·51	·09	·69	·19	·07	—	·02	·30	—	·46	·02	·37	·12
·38	—	—	·52	·42	·14	·42	·59	·77	·31	2·05	·31	·07	—	·28	·21	·03	2·86	—	·66	·52
·38	—	—	·60	1·07	·22	·52	·54	1·00	·22	1·26	·38	·07	·05	·36	·21	·07	2 63	—	·59	1·69
·13	·05	—	·26	·75	·08	·59	·36	1·29	·21	1·65	·44	·03	—	·08	·44	·15	1·26	—	·49	·41
·36	·04	—	·55	·96	·69	·55	·50	1·24	·28	1·66	·32	·14	·14	·37	·68	·15	3·82	·01	·58	·75
·24	·05	—	·47	·78	·23	·52	·38	·72	·15	1·33	·31	·06	·04	11	·40	·06	1·17	·01	·41	·28
·30	·05	—	·52	·87	·43	·53	·43	·94	·21	1·47	·32	·10	·08	·22	·52	·11	2·32	·01	·48	·49
·13	—	—	·56	1·35	·27	·70	·17	1·52	·39	1·52	·15	-	—	·09	·43	—	1·65	—	·35	·96
·81	·12	·01	1·18	·85	·49	·80	·76	1·37	·25	2·47	·22	·19	·15	·24	1·24	·08	2·21	·06	73	·78
·27	·33	—	·65	·84	1·03	·91	·53	1·45	·32	1·80	·25	·08	—	·09	1·16	·08	1·01	—	·60	·59
·56	·20	—	·96	·82	·09	·80	·71	1·16	·27	2·26	·20	·11	·02	·05	1·01	·02	1·16	—	·60	·40
·57	·18	·00	·94	·88	·53	·82	·65	1·37	·28	2·19	·22	·13	·06	·16	1·17	·06	1·67	·03	·64	·68
·36	·13	·00	·68	·55	·17	·40	·33	·61	·18	1·39	·25	·06	·03	·10	·39	·05	1·08	·02	·38	·39
·41	·14	·00	·75	·62	25	49	·40	·93	·20	1·57	·24	·07	·04	·11	·56	·06	1·21	·03	·44	·45

Towns which in 1871 or in 1881 had a population of 10,000 or upwards.

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TABLE III.—

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881), situated in the respective Unions	Area in Statute Acres	Population in 1881	Number per 1,000 of Population Temporarily Diseased									
				At their own Homes	In Infirmaries and General and Special Hospitals	In Workhouse Hospitals	Total	Zymotic					
								Smallpox	Measles	Scarlatina	Diphtheria	Whooping-cough	
PROVINCE OF ULSTER													
Armagh, - -	Armagh, - -	154,247	62,855	4·12	·75	1·28	6·15	·05	—	—	—	·17	
Belfast, - -	Belfast, - -	48,372	239,283	3·29	1·25	4·70	9·24	·03	·02	·01	—	·12	
Lisburn, - -	Lisburn, - -	119,871	55,618	3·96	·77	2·03	6·76	—	·13	—	—	·07	
Londonderry, -	Londonderry, -	144,127	59,611	2·47	1·32	·91	4·70	—	—	—	—	—	
Lurgan, - -	Lurgan, - -	79,943	62,982	3·48	—	3·11	6·59	—	·02	·02	—	·10	
Newry, - -	Newry, - -	137,974	67,722	3·07	·42	·81	4·30	—	·05	·04	—	—	
Total Civic Unions,* - -		684,534	549,071	3·36	·90	2·96	7·22	·02	·03	·01	—	·06	
Rural Unions,† - -		4,720,302	1,208,061	4·01	·23	1·53	5·77	·00	·01	·04	·00	·04	
Total of Province, -		5,404,836	1,757,132	3·81	·43	1·98	6·22	·01	·01	·03	·00	·05	
PROVINCE OF CONNAUGHT.													
Galway, - -	Galway, -	197,467	43,101	2·62	1·07	2·48	6·17	—	—	—	—	—	
Sligo. - -	Sligo, - - -	143,523	46,063	2·74	1·30	2·32	6·36	—	—	·02	—	—	
Total Civic Unions,* - -		340,990	89,164	2·68	1·19	2·40	6·27	—	—	·01	—	—	
Rural Unions,† -		3,868,207	727,510	3·67	·17	2·55	6·39	·00	·06	·05	·00	·03	
Total of Province, -		4,209,197	816,674	3.56	·28	2·53	6·37	·00	·05	·05	·00	·01	
IRELAND.													
Total Civic Unions,* - -		2,178,543	1,482,159	2·99	2·09	5·51	10·51	·02	·03	·07	·00	.04	
Rural Unions,† - -		18,147,666	3,692,677	3·65	·29	2·70	6·64	·00	·05	·06	·00	·04	
Total, - -		20,326,209	5,174,836	3·44	·81	3·50	7·75	·01	·04	·06	·00	·04	

* Unions or Superintendent Registrars' Districts containing
† All Unions other than those specified in Note (*).

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continued.

Nature of Illness and Rate per 1,000																				
Diseases							Constitutional Diseases		Local Diseases of							Developmental Diseases			Injuries	Unspecified or Ill-defined
Fever	Diarrhœa, Dysentery	Cholera	Principal Zymotic Diseases	Rheumatism	Other Zymotics	Consumption	Others	Nervous System	Circulatory Organs	Respiratory Organs	Digestive Organs	Urinary Organs	Generative Organs	Organs of Locomotion	Integumentary System	Childbirth	Debility, Old Age	Others		
·11	—	—	·33	·61	·16	·61	48	·61	·09	1·08	·19	·09	·02	·12	·31	·05	·77	·02	·39	·22
·09	·03	—	·30	·66	·23	·71	·33	·71	·14	1·78	·39	.11	·07	·19	1·01	·07	1·37	·01	·53	·63
·15	·09	—	·44	·41	·22	·47	·34	·86	·20	1·35	·32	·20	—	·13	·65	·02	·43	—	·49	·23
·37	·02	—	·39	·49	·42	·57	·40	·35	·07	·55	·17	·15	·07	·03	·15	·02	·37	·05	·37	·08
·06	·14	—	·34	·63	·11	·78	·35	·54	·14	1·03	·37	·11	—	·02	·41	·08	·95	·03	·35	·35
·07	·01	—	·17	·56	·21	·50	·22	·31	·09	·74	·13	·06	—	·07	·34	—	·39	—	·39	·12
·12	·04	—	·31	·60	·22	·64	·35	·60	·13	1·31	·30	·12	·04	·12	·65	·05	·93	·01	·45	·39
·20	·04	—	·33	·59	·09	·50	·39	·57	·14	·92	·28	·09	·01	·10	28	·04	·74	·01	·44	·25
·18	·04	—	·33	·59	·13	·55	·38	·58	·13	1·04	·29	·10	·02	·11	·39	·04	·80	·01	·44	·29
·51	·02	—	·53	·37	·09	·37	·28	·93	·12	1·23	·12	·09	·05	·16	·44	·02	·49	—	·56	·32
·23	·07	—	·31	·43	·07	·44	·30	·70	·04	1·13	·39	·15	·04	·15	·41	·09	·78	·02	·54	·37
·36	·04	—	·41	·40	·08	·40	·29	·81	·08	1·18	·26	·12	·04	·16	·43	·06	·64	·01	·55	·35
·33	·07	—	·53	·68	·08	·41	·30	·66	·12	1·16	·26	·05	·01	·11	·37	·05	·87	·02	·43	·28
·33	·07	—	·51	·65	·08	·41[1]	30	·68	·12	1·17	·26	·06	·01	·11	·37	·05	·84	·02	·45	·28
·32	·07	·00	·55	·79	·45	.63	·46	1·00	·21	1·61	·29	·13	·08	·22	·75	·09	2·12	·01	·54	·58
·28	·07	·00	·50	·63	·14	·46	·35	·68	·15	1·18	·27	·07	·02	·10	·35	·06	·94	·02	·42	·30
·29	·07	·00	·51	·67	·23	·51	·38	·79	·16	1·30	·28	·09	·04	·14	·46	·06	1·28	·02	·45	·38

Towns which in 1871 or in 1881 had a population of 10,000 or upwards.

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TABLE IV.—*Showing the Number and Diseases of Persons who laboured under Superintendent Registrars' Districts containing Towns with a Population of (3) the remaining portion of each Province; (4) the total of each Province;*

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881), situated in the respective Unions	Area in Statute Acres	Population in 1861	Number of Temporarily Diseased to every 1,000 of the Population	Number of Temporarily Diseased: At their own Homes	In Infirmaries and General and Special Hospitals	In Workhouse Hospitals	Total	Zymotic: Smallpox	Measles	Scarlatina	Diphtheria	Whooping-cough	Fever
PROVINCE OF LEINSTER.														
Drogheda, -	Drogheda, - -	99,062	31,630	6·0	111	—	79	190	1	—	6	—	—	4
Dublin North, } Dublin South, }	Dublin, Rathmines and Rathgar, and Pembroke.	41,256 / 48,200	142,981 / 202,264	17·4 / 15·6	194 / 408	663 / 999	1,630 / 1,754	2,487 / 3,161	— / 4	4 / 6	9 / 32	1 / 1	— / 1	58 / 96
Dundalk, -	Dundalk, - -	104,629	43,193	4·5	136	38	19	193	—	—	—	—	—	3
Kilkenny, -	Kilkenny, - -	110,943	28,733	10·2	110	44	138	292	—	2	2	—	—	11
Rathdown, -	Kingstown, - -	61,513	57,913	11·4	150	105	408	663	8	—	3	—	2	22
Wexford, -	Wexford, - -	126,201	38,745	8·5	163	26	140	329	—	—	3	—	—	5
Total Civic Unions,* - -		591,904	545,459	13·4	1,272	1,875	4,168	7,315	13	12	55	2	3	199
Rural Unions,† - -		4,114,598	710,562	7·5	2,680	406	2,224	5,310	10	60	48	—	14	172
Total of Province, -		4,706,502	1,256,021	10·1	3,952	2,281	6,392	12,625	23	72	103	2	17	371
PROVINCE OF MUNSTER.														
Clonmel, -	Clonmel, - -	87,707	23,009	10·0	100	9	123	231	—	—	9	—	1	3
Cork, - -	Cork and Queenstown.	169,732	145,216	14·2	453	454	1,150	2,057	1	13	12	—	9	117
Limerick, -	Limerick, - -	177,986	75,051	11·4	172	90	593	855	—	—	4	—	—	20
Waterford, -	Waterford, - -	125,730	55,189	10·7	245	53	289	587	—	3	8	—	—	31
Total Civic Unions,* - -		561,115	298,465	12·5	970	606	2,154	3,730	1	16	33	—	10	171
Rural Unions,† - -		5,444,559	1,046,544	7·3	3,282	288	4,023	7,593	1	61	89	2	58	378
Total of Province, -		6,005,674	1,345,009	8·4	4,252	894	6,177	11,323	2	77	122	2	68	549

* Unions or Superintendent Registrars' Districts containing Towns
† All Unions other than those specified in Note (*).

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Temporary Diseases on the Night of 3rd of April, 1881, *in* (1) *those Unions or* 10,000 *or upwards*; (2) *the Group of such Unions in each Registration Province*; *with corresponding Rates for all Ireland.*

Nature of Illness and Number of Cases																		
Diseases					Constitutional Diseases		Local Diseases of							Developmental Diseases			Injuries	Unspecified or ill-defined
Diarrhœa, Dysentery	Cholera	Principal Zymotic Diseases	Rheumatism	Other Zymotics	Consumption	Others	Nervous System	Circulatory Organs	Respiratory Organs	Digestive Organs	Urinary Organs	Generative Organs	Organs of Locomotion	Integumentary System	Childbirth	Debility, Old Age	Others	
2	—	13	15	3	20	8	12	7	45	4	3	—	3	5	2	26	—	9	15
8	—	80	190	132	91	54	189	54	308	58	26	28	56	162	35	830	—	94	100
8	—	148	209	211	100	139	321	56	324	59	35	45	109	157	32	924	3	126	163
2	—	5	19	13	21	12	22	4	30	8	3	—	1	13	—	20	1	16	5
—	—	15	12	4	12	17	22	9	59	9	2	—	8	6	1	82	—	19	15
—	—	35	62	13	30	31	58	13	73	20	4	3	21	12	4	152	—	34	38
2	—	10	29	3	23	14	50	8	64	17	1	—	3	17	6	49	—	19	16
22	—	306	536	379	297	275	674	151	903	175	74	76	201	372	80	2,083	4	317	412
39	—	343	557	167	372	268	509	107	943	221	54	25	76	285	58	830	5	292	198
41	—	649	1,093	546	669	543	1,183	258	1,846	396	128	101	277	657	138	2,913	9	609	610
—	—	13	31	5	16	4	35	9	35	3	—	—	2	10	—	38	—	8	22
18	1	171	124	71	116	110	199	36	359	32	27	21	35	195	12	321	9	106	113
25	—	49	63	77	68	40	109	24	135	19	6	—	7	87	6	76	—	45	44
11	—	53	45	5	44	39	64	15	125	11	6	1	3	56	1	64	—	33	22
54	1	286	263	158	244	193	407	84	654	65	39	22	47	348	19	499	9	192	201
132	1	722	574	180	414	345	844	164	1,460	258	63	27	101	404	57	1,131	26	398	405
186	2	1,008	837	338	658	538	1,251	268	2,114	323	102	49	148	752	76	1,630	35	590	606

which in 1871 or in 1881 had a population of 10,000 or upwards.

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TABLE IV.—

Poor Law Unions or Superintendent Registrars' Districts	Towns of 10,000 or upwards (in 1871 or in 1881), situated in the respective Unions	Area in Statute Acres	Population in 1881	Number of Temporarily Diseased to every 1,000 of the Population	Number of Temporarily Diseased				Zymotic					
					At their own Homes	In Infirmaries and General and Special Hospitals	In Workhouse Hospitals	Total	Smallpox	Measles	Scarlatina	Diphtheria	Whooping-cough	Fever
PROVINCE OF ULSTER.														
Armagh, - -	Armagh, - -	154,247	63,855	6·2	263	48	82	393	3	—	—	—	11	7
Belfast, - -	Belfast, - -	48,372	239,283	9·2	788	299	1,125	2,212	7	4	3	—	29	22
Lisburn, - -	Lisburn, - -	119,871	55,618	6·8	220	43	113	376	—	7	—	—	4	8
Londonderry, -	Londonderry, -	144,127	59,611	4·7	147	79	54	280	—	—	—	—	—	22
Lurgan, - -	Lurgan, - -	79,943	62,982	6·6	219	—	196	415	—	1	1	—	6	4
Newry, - -	Newry, - -	137,974	67,722	4·3	208	28	55	291	—	3	3	—	—	5
Total Civic Unions,* - -		684,534	549,071	7·2	1,845	497	1,625	3,967	10	15	7	—	50	68
Rural Unions,† - -		4,720,302	1,208,061	5·8	4,849	271	1,849	6,969	1	8	45	1	50	243
Total of Province, -		5,404,836	1,757,132	6·2	6,694	768	3,474	10,926	11	23	52	1	100	311
PROVINCE OF CONNAUGHT.														
Galway, - -	Galway, - -	197,467	43,101	6·2	113	46	107	266	—	—	—	—	—	22
Sligo. - -	Sligo, - -	143,523	46,063	6·4	126	60	107	293	—	—	1	—	—	10
Total Civic Unions,* - -		340,990	89,164	6·3	239	106	214	559	—	—	1	—	—	32
Rural Unions,† - -		3,868,207	727,510	6·4	2,668	121	1,858	4,647	3	41	39	2	12	228
Total of Province, -		4,209,197	816,674	6·4	2,907	227	2,072	5,206	3	41	40	2	12	270
IRELAND.														
Total Civic Unions,* - -		2,178,543	1,482,159	10·5	4,326	3,084	8,161	15,571	24	43	96	2	63	470
Rural Unions,† - -		18,147,666	3,692,677	6·6	13,479	1,086	9,954	24,519	15	170	221	5	134	1,033
Total, -		20,326,209	5,174,836	7·7	17,805	4,170	18,115	40,090	39	213	317	7	197	1,501

* Unions or Superintendent Registrars' Districts containing Towns
† All Unions other than those specified in Note (*).

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continued.

Nature of Illness and Number of Cases																		
Diseases					Constitutional Diseases		Local Diseases of							Developmental Diseases			Injuries	Unspecified or Ill-defined
Diarrhœa, Dysentery	Cholera	Principal Zymotic Diseases	Rheumatism	Other Zymotics	Consumption	Others	Nervous System	Circulatory Organs	Respiratory Organs	Digestive Organs	Urinary Organs	Generative Organs	Organs of Locomotion	Integumentary System	Childbirth	Debility, Old Age	Others		
—	—	21	39	10	39	31	39	6	69	12	6	1	8	20	3	49	1	25	14
8	—	73	158	55	169	79	170	35	427	93	27	17	45	242	17	327	2	126	150
5	—	24	23	12	27	19	48	11	75	18	11	—	7	36	1	24	—	27	13
1	—	23	29	25	34	24	21	4	33	10	9	4	2	9	1	22	3	22	5
9	—	21	40	7	49	22	34	9	65	23	7	--	1	26	5	60	2	22	22
1	—	12	38	14	34	15	21	6	50	9	4	—	5	23	—	26	—	26	8
24	—	174	327	123	352	190	333	71	719	165	64	22	68	356	27	508	8	248	212
51	—	399	712	113	607	475	691	162	1,112	342	105	13	124	334	50	892	14	529	295
75	—	573	1,039	236	959	665	1,024	233	1,831	507	169	35	192	690	77	1,400	22	777	507
1	—	23	16	4	16	12	40	5	53	5	4	2	7	19	1	21	—	24	14
3	—	14	20	3	20	14	32	2	52	18	7	2	7	19	4	36	1	25	17
4	—	37	36	7	36	26	72	7	105	23	11	4	14	38	5	57	1	49	31
51	—	386	495	60	302	216	479	87	848	188	37	4	77	268	39	632	14	314	201
55	—	423	531	67	338	242	551	94	953	211	48	8	91	306	44	689	15	363	232
104	1	803	1,162	667	929	684	1,486	313	2,381	428	188	124	330	1,114	131	3,147	22	806	856
273	1	1,850	2,338	520	1,695	1,304	2,523	540	4,363	1,009	259	69	378	1,291	204	3,485	59	1,533	1,099
377	2	2,653	3,500	1,187	2,624	1,988	4,009	853	6,744	1,437	447	193	708	2,405	335	6,632	81	2,339	1,955

which in 1871 or in 1881 had a population of 10,000 or upwards.

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From Table I. the following statement may be extracted:—

	Death-rate per 10,000 Inhabitants		
	Phthisis	Diseases of Respiratory Organs	From both combined
Civic Districts - -	27·7	38·4	66·1
Rural Districts - -	16·4	22·6	39·0

The mean of death-rate for lung diseases in Irish town populations over country populations is no less than 27·1 per 10,000.

If Tables III. and IV. be examined similar conditions will be found to exist in the sickness returns, as shown in the following statement:—

	Civic Districts		Rural Districts	
	Sickness		Sickness	
	No.	Rate per 10,000	No.	Rate per 10,000
Diseases of Respiratory Organs - -	2,381	1·61	4,363	1·18
Phthisis - - -	929	·63	1,695	·46
Total - -	3,310	2·24	6,058	1·64

Here we see that the rate of prevalence of diseases of the lungs on the census night was 2·24 per 10,000 of the population in towns, as compared with 1·64 in the country districts. From the foregoing statement it is evident that the predominating causes of sickness and death in Irish towns are lung diseases, not, as many suppose, infective fevers. No doubt infective fevers have a great influence and are more prevalent in towns than in country districts, whether measured by the average decennial death-rate or by the census disease-rate. The civic death-rate from the principal zymotic diseases was 32·2, and the rural 17·7 per 10,000, while the relative disease-rates were respectively 5·5 and 5·0 per 10,000 on persons living on census night. This would point to a but slightly greater prevalence of infective diseases in town than in country, but a much higher rate of mortality among the affected persons. This, as regards the mortality, is exactly what might be expected

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from the relatively higher mortality from lung diseases already discussed.

Time would not permit me to analyse and remark upon any considerable number of the details set out in the tables and illustrated in the diagrams. I have already treated of some of the principal features. There are a few, however, which I think it well to point to more particularly. As already shown, the deaths from each cause or group of causes dealt with in the tables and diagrams, except childbirth, are higher in civic than in rural districts. First, to deal with the exception. I would here quote from the Decennial Summary of Births and Deaths, as follows :[a]—

"It is difficult completely to separate the statistics of deaths from puerperal fever and from childbirth, as they may be all put down to a common cause—namely, parturition, and no doubt a considerable number of deaths from puerperal fever, especially in the country, are returned simply as deaths from 'Childbirth.'

"The total number of deaths under these two heads was 9,295, or at the annual rate of 15·3 per 1,000 of married women within the child-bearing ages (namely, 15 to 55). The deaths from puerperal fever numbered 3,543, or at the rate of 5·8 per 1,000 of married women at the child-bearing ages, and the other deaths from childbirth numbered 5,752, or at the rate of 9·5 per 1,000 at the same age periods. The deaths from puerperal fever were at the rate of 2·5 per 1,000, and those from childbirth at the rate of 4·1 per 1,000 of the births registered, the two combined being equal to 1 in 151, or at the rate of 6·6 per 1,000 of the births registered.

"The Abstract (pages 98–101) shows that in every Union in Ireland deaths from puerperal fever were recorded. Similarly, deaths from other forms of the affections of childbirth are also recorded in all unions. Tables X. and XI. show the relative distribution of deaths from this group of disease between town and country. A result is shown by this comparison which differs remarkably from the results obtained where the town and country death-rates are compared for any of the other diseases or groups of

[a] Loc. cit. P. 25.

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diseases dealt with in these tables. The death-rates to the population from puerperal fever and childbirth combined are almost the same in town and country—namely, 1·8 and 1·7 per 10,000 respectively; taken separately, however, the death-rate from puerperal fever, which is an infective disease, is slightly higher in town than in country, while in other forms of death from childbirth the mortality is greater in the country than in town.

"If the number of these deaths be compared with the number of births registered, we have the result as set out in the following statement:—

"TEN YEARS, 1871–80.

"'PUERPERAL FEVER' and 'CHILDBIRTH' in 'CIVIC' and in 'RURAL' UNIONS.

"In Civic Unions deaths from puerperal fever			=	1 in 355 or 2·8 per 1,000 *births* registered.		
„ Rural	„	„	=	„ 415 or 2·4	„	„
„ Civic	„	childbirth	=	„ 278 or 3·6	„	„
„ Rural	„	„	=	„ 232 or 4·3	„	„
„ Civic	„	puerperal fever and childbirth combined	=	„ 156 or 6·4	„	„
„ Rural	„	„	=	„ 149 or 6·7	„	„
"Total deaths from puerperal fever			=	„ 396 or 2·5	„	„
„		'childbirth'	=	„ 244 or 4·1	„	„
„		puerperal fever and childbirth combined	=	„ 151 or 6·6	„	„

"From this it appears that the relation of deaths from puerperal fever and childbirth combined to births registered, is for the whole of Ireland at the rate of 1 in 151 or 6·6 per 1,000, for puerperal fever alone it is 1 in 396 or 2·5 per 1,000, and for childbirth 1 in 244 or at the rate of 4·1 per 1,000 births. If, however, town and country are compared it appears that the mortality from puerperal fever in town districts is at the rate of 1 in 355 or 2·8 per 1,000 births, while in country districts it is 1 in 415 or 2·4 per 1,000, being slightly less. On the other hand, the deaths from 'childbirth' are proportionately less in town than in country; in the civic districts the rate is one in 278 or at the rate of 3·6 per 1,000, while in country districts it is 1 in 232 or 4·3 per 1,000 births. If the two be taken together the result is that deaths from parturition are at the rate of 6·4 per 1,000 births in town as compared with

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6·7 in country districts. It is clear that this excessive mortality from parturition can be attributed only to the less favourable circumstances under which lying-in women are treated in the country as compared with town. Many, no doubt, do not obtain any medical attendance, and in a considerable number of cases probably the poor women have no skilled assistance whatever at this critical time either in the way of a medical attendant or a properly trained midwife. When the generally low health of town populations is contrasted with the health of country populations a greater mortality from parturition (as from various forms of disease) might fairly be expected in the former than in the latter, and therefore it is quite clear that the explanation must be in the comparative neglect with which lying-in women are treated in the country parts of Ireland."

Another point worthy of note is that in the districts containing the largest towns, especially Dublin, Belfast, and Cork, the more important causes of death prevail to the greatest degree. This is especially notable in the case of Dublin, where in every case referred to in the tables, except in three groups, the mortality exceeds that of any other town district in Ireland. These three exceptions are whooping-cough, diarrhœa, and phthisis, in each of which the death-rate is higher in Belfast than in Dublin. In the case of whooping-cough the excess in Belfast as compared with Dublin may be accounted for to a great extent by the fact that the disease is principally one of childhood, and is most frequently fatal in very young children, of whom there are a greater proportion in Belfast than in Dublin, the former having a higher birth-rate than the latter. The excess of diarrhœa may be to some extent accounted for in a similar manner, but may also be somewhat influenced by the superior water supply of Dublin. The higher mortality from phthisis in Belfast must, I think, to a great extent be attributed to the occupations of the people, and the consequent large number of persons, especially young girls, employed as factory hands. It is a remarkable fact that while the deaths from phthisis were 33·4 and 30·0 for the Dublin districts, and 38·2 for Belfast, the deaths from diseases of the respiratory organs were 52·9 and 53·3 in Dublin,

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and only 49·9 in Belfast. This would point to the tendency of pulmonary affections in Belfast to assume the phthisical type more readily than in Dublin.

The foregoing remarks are intended more to draw attention to this important subject than to treat of the details set out in the tables and illustrated in the diagrams. I would refer those who wish to follow up the subject in detail to the "Supplement," &c., containing the decennial summary already quoted and referred to.

I have not thought it advisable to enter into any comparison between the death-rates of Irish town populations and those of the populations of towns in the sister kingdoms or foreign lands. Such a comparison would be of great interest, and I hope will be instituted and worked out in detail. I am sorry to say that I anticipate that the result of such an inquiry would not be much to the credit of this country, and would fully confirm the observations made by Dr. H. C. Tweedy (Secretary to this Sub-Section), in a very suggestive paper read by him at the Cork meeting of the British Medical Association in 1879. It is much to be regretted that Dr. Tweedy's paper was not published at the time. Perhaps its author may be persuaded to further elaborate, extend, and bring up to date the statistics upon which he grounded his remarks on that occasion.

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COMPULSORY NOTIFICATION OF INFECTIOUS DISEASE.

By ARCHIBALD H. JACOB, M.D., F.R.C.S.;
Ophthalmic Surgeon, Richmond Hospital;
Professor of Ophthalmic Surgery, Royal College of Surgeons.

[Read in the Sub-Section of State Medicine, May 14, 1885.]

I am impelled to bring the subject of the compulsory notification of infectious disease under the notice of the Academy by the fact that at the present moment a Bill—the Burgh Police and Health (Scotland) Bill—is under the consideration of a Select Committee of the House of Lords, and that, if it should become law, the system of compelling the attending physician to notify to the sanitary authority every case of infective disease which occurs within his practice, will be extended throughout almost the whole of Scotland. This Bill was introduced last year by the Lord Advocate, and though its provisions have already undergone, and and are about to undergo, the pretence of investigation, the medical profession, who are the only witnesses who can afford reliable information on the subject, and whose interests are most vitally concerned in this sort of legislation, have not been heard at all—nay, they have been refused a hearing; and thus a series of statements which are unsupported by facts or professional experience have been accepted by Parliament as uncontradicted, and therefore presumably true.

As a preliminary to any statement of the facts, or of my opinion upon the propriety of legalising compulsory notification, it is necessary that I shall briefly, for the benefit of those who are not familiar with the system, state the nature of it.

The law to compel, under penalty of fine or imprisonment, the attending physician or the person in whose house the infectious

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disease exists, or both of them, to send information of its outbreak to the sanitary authority of the district, was introduced for the first time in the town of Bolton in 1877. Since then it has been extended to twenty-two towns in England and Scotland, by means of clauses introduced into Gas and Water Bills; but no general Bill has ever been introduced to Parliament, nor has the expediency of compulsory notification, nor its prophylactic value, ever been adequately considered, either by the Legislature or by the populations of the towns to which it has been applied, or by the physicians through whose forced services the system was to be carried out.

METHODS OF NOTIFICATION.

There are at present in force four different methods by which information of the occurrence of infectious disease is conveyed to the sanitary authority, viz.:—

(*a.*) *Direct Medical Notification.*—Where the physician in attendance is obliged to report the case direct *to the Medical Officer of Health*, or his representative, with or without the consent of the patient.

(*b.*) *Indirect Medical Notification.*—Where the physician is obliged to hand *to the custodian of the patient* a written notice of the nature of the disease, which notice the custodian is responsible to send forthwith to the Medical Officer of Health, or his representative, the custodian being only responsible to notify on his own account when there is no medical attendant.

(*c.*) *Dual Notification.*—Where the physician is obliged to send notice and the custodian also, even though a physician is in attendance, and has already reported the case. This system, as will be observed, exists in certain towns as a part of direct medical notification, and is, in fact, a means of securing a report from one party, if the other neglects the duty.

(*d.*) *Householder Notification.*—Where the custodian of the patient is *alone* responsible to send the notice, though there may be a physician in attendance.

Twenty-three towns in England and Scotland are subject to one or other of these forms of the law.

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Direct Medical Notification exists in—

* Barrow-in-Furness.
Birkenhead.
Blackburn.
* Blackpool.
* Bolton.
Burton-on-Trent.
* Derby.
* Huddersfield.
* Jarrow.
* Lancaster.
* Leicester.
* Llandudno.
* Manchester.
* Oldham.
Preston.
* Reading.
* Rotheram.
* Stafford.
* Stalybridge.
* Warrington.
Edinburgh.

Indirect Medical Notification exists in—

* Bradford. | Norwich. | * Nottingham.

Dual Notification exists in those towns marked above with an asterisk, and has been extended to the following towns by private Bills last year, but has not yet been put in force therein:—Accrington, Chadderton, Macclesfield, Newcastle, Dundee.

Householder Notification exists only in Greenock.

For the extension of the notification system throughout the kingdom generally, Bills have been promoted in successive Sessions since 1881—by Mr. Hastings, M.P., on behalf of the Social Science Association, for England and Wales; by Mr. Dwyer Gray, M.P., for Ireland; and a compromise Bill, by Mr. Meldon, at the instance of the Irish Medical Association, for Ireland also.

These Bills have all been dropped, but Sir Charles Dilke has stated, on behalf of Government, that they hope before long to take the subject in hand with a view to the enactment of a comprehensive measure to rule the entire kingdom. Meanwhile, the system of shuffling compulsory notification law through Parliament, hidden away amongst the clauses of private Bills which dealt with gas, water, tramway, and other such like local improvements, had come to be so gross an abuse that, in the Session of 1883, Sir William Harcourt appointed a special Select Committee to examine into the contents of such Bills, and he refused to allow any private

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measure to pass until it had passed the scrutiny of that committee. Since then the compulsory notification system has, by means of private Bill legislation, been extended to several towns; but I shall hereafter make it clear to you that these extensions have been assented to by the committee not only without due inquiry, but upon refusal of the demands by the profession to be heard in evidence against the notification system, and I must, therefore, beg of you to keep your minds clear of the assumption that the case for compulsory notification has been tried out before a Parliamentary tribunal, and that the verdict has been in favour of the system.

The legal expression of compulsory notification in its most approved form is to be found in Mr. Hastings' Bill, of which the chief working clauses are as follows:—

"3. Infectious disease shall include smallpox, cholera, typhus, typhoid, scarlet, relapsing, continued, and puerperal fevers, and diphtheria and such other diseases as the sanitary authorities may from time to time declare to be infectious.

"4. Every medical practitioner attending on or called in to visit such inmate shall, on becoming aware that such inmate is suffering from any infectious disease, forthwith fill up, sign, and deliver, or send to the Medical Officer of Health at his office a certificate or declaration stating according to the form so prescribed the name of such inmate, the situation of such building, the name of such occupier or person, and the nature of the infectious disease from which, in the opinion of the medical practitioner, such inmate is suffering.

"5. The sanitary authority shall pay to every registered medical practitioner who shall, in pursuance of this section, duly make and give any such certificate or declaration a fee of *two shillings and sixpence* for each certificate or declaration in respect of cases occurring in his private practice, and a fee of *one shilling* for each such certificate or declaration in respect of cases occurring in his practice as a medical officer to any public body or institution.

"6. And any person who shall wilfully offend against this enactment shall for every such offence be liable to a penalty not exceeding *forty shillings*."

The foregoing phrases, quoted from Mr. Hastings' Bill, are a mild and inoffensive expression of the compulsory notification law.

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The law as it actually exists in twenty-three towns by no means adopts this formula, for it varies in each town according to the caprice of the Medical Officer of Health and Town Clerk, who draughted the clauses; and as specimens of coercion legislation some of these provisions, to which I shall presently ask your attention, are astonishing productions.

My purpose this evening is to move you, if possible, to use every influence, in and out of season, to bar the further extension of the compulsory notification system, until a thorough, open, straightforward inquiry be held into its working in the towns in which it has hitherto been in force; and I hope to convince you—

1. That this law is altogether unconstitutional and without precedent.

2. That it is necessarily and intrinsically calculated to increase the dangers which it is intended to abate.

3. That, however desirable it may be, it can never be generally enforced.

4. That, as a matter of fact, there is no evidence that it has produced, or promises to produce, any beneficial effect whatever upon the public health; and

5. That it is grossly oppressive upon the medical profession.

In a scientific tribunal such as the Academy, it would be out of place to dwell upon notification law from the point of view of its consistency with constitutional liberty and legislative principle, but I cannot pass this consideration without notice, because the wider extension of this law turns much more upon the opposition raised to it by the champions of constitutional liberty than upon any consideration of public health. In the progress of notification Bills through the House of Commons or through Select Committees, the voice of our profession has, I regret to say, been as silent as if we did not exist. Not so the voice of the constitutionalists, which, through the agency of Messrs. Hopwood, Burt, and other representatives of an organisation called the "Vigilance Association for the Defence of Personal Rights," has been loudly heard. The medical profession is, in fact, indebted for protection not to its own energies or influence (which in this, as in all other Parliamen-

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tary contests, may be stated as *nil*), but to the energies and influence of the anti-vaccinationists, anti-vivisectionists, and anti-contagious diseases Acts fanatics.

It will be observed that almost all the notification Bills propose, in one form or other, to compel the medical attendant to communicate to the health officer of the district, for public uses, the information acquired by him in his capacity of confidential medical adviser, and it visits him with a fine and with imprisonment in default of payment, if from any cause he omits to do so. Furthermore, several of these Acts, and especially the Bills of Messrs. Hastings and Gray, while imposing this responsibility and penalty upon the physician, entirely relieve the custodian of the patient of any responsibility whatsoever in every case in which a physician has visited.

Now, I assert that there is no precedent in English law for compulsory notification by the physician, and that the advocates of compulsory notification labour under the difficulty of having to justify a perfectly new departure from the principles of justice and liberty, which have heretofore guided the legislation of our country. It strikes at two well-recognised principles of equity—

1. That the person who is responsible for the existence of a nuisance dangerous to his neighbour shall be also the person responsible for the abatement of that nuisance.

2. That the information acquired by any person in the performance of a confidential function shall be confidential and inviolate, and shall not be applicable to public uses, save upon extreme public necessity.

NO PRECEDENT FOR NOTIFICATION BY THE PHYSICIAN.

In conformity with the first of these principles, I insist that the custodian of the infected patient, and he alone, should be held liable to inform the sanitary authority of the nature of the disease; and I challenge the advocates of this law to show, under English law, any analogue for legislation such as that now proposed.

What has the law hitherto said upon this subject? When a *nidus* of communicable disease, either in the form of a dangerous

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nuisance, such as a manure heap, an open sewer, or the noxious emanation of a manufactory, is found to exist, or when an infectious disease occurs amongst the lower animals, who is held responsible to report the existence of a nuisance or of the infection? Let the Public Health Acts and the Contagious Diseases (Animals) Act answer the question. Under the Public Health Act (sec. 108) it is the duty of the "*person by whose act, default, or sufferance the nuisance arises or continues*, or, if such person cannot be found, the owner or occupier of the premises," to abate it (sec. 110).

Again, in dealing with infectious disease, what does the law say?—"where any sanitary authority are of opinion . . . . that the cleansing and disinfecting of any house . . . . would tend to prevent or check infectious disease, it shall be the duty of such authority to give notice . . . . *to the occupier*" (sec. 137). And, again, in secs. 144–5–6 the responsibility and penalty for letting infected houses and sending infected children to school is imposed solely upon the "person who shall knowingly or negligently" do so.

The Contagious Diseases (Animals) Act is still more definite on this point. It enacts (sec. 31) that "Every *person having in his possession or under his charge* an animal affected with disease shall . . . . give notice of the fact of the animal being so affected." Again (sec. 61), "If any person, without lawful authority or excuse, *proof whereof shall lie on him*, fails to give notice of the disease with all practicable speed," he shall be liable to a penalty of £20. It will be seen, therefore, that the Act simply orders the owners of any infected cattle to report the occurrence, or take the consequences, punishing them for default if they omit to do so, and compensating them for loss if they do. Furthermore, it will be observed that these Acts leave to the sanitary authority the duty of detecting the existence of infective disease and punishing it, and I am not aware that any enthusiast has ever proposed to fine and imprison a veterinary surgeon or a neighbouring farmer for omitting to supply the sanitary authority with information.

In no Act of Parliament that I ever heard of is there any proposal to be found such as the compulsory notificationists put

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forward, that some third party, who is perfectly guiltless of offence against the public weal, but who happens to become confidentially cognisant of the commission of an offence, shall be fined and imprisoned for not reporting the guilt of the offender, while the offender himself is not visited with any punishment whatever.

To the inapplicability to public uses of the information obtained in the discharge of confidential functions I shall presently refer.

CONCEALMENT OF DISEASE UNDER THE NOTIFICATION SYSTEM.

I pass on now to endeavour to show you that compulsory notification as it now exists is, by its nature, calculated to increase the evils which it aims at abating—to demoralise the public and the medical profession, and to encourage the concealment and consequent dissemination. Assuming the advantage to be gained if the sanitary authority of each district could receive early knowledge of each infectious case, I assert that the most certain way to prevent this object being gained is to compel the physician to act as sanitary detective.

How does the world wag where notification law does not exist? A child shivers and sickens with no one knows what disease; the father instantly, on suspicion of an infectious fever, runs for the doctor, who, recognising that the disease is, say, scarlatina, or small-pox, at once, and without hesitation, gives the parent his opinion, advises strongly the necessary isolation, prescribes the proper treatment, and warns the family the danger they and their neighbours run if precautions are neglected. The parent, animated by love of his child, and by apprehension lest the disease may extend to others of his family, and fearful that his business will be greatly injured if the house becomes saturated with infective poison, or if the disease is communicated to others, does his best to act upon the advice of the physician, and in the great majority of cases every purpose which compulsory notification is intended to serve is attained. There is, in fact, in most cases a fully sufficient paternal and self-interested stimulus at work in favour of isolation and other sanitary precautions, and there are only needed the necessary knowledge that the disease is infective,

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and the necessary advice as to what should be done. This knowledge and advice in the very earliest stage of the disease are invaluable, and any system which discourages the intervention of the physician at such a moment, as compulsory notification does, cannot but prove disastrous both to the patient, the family, and the public.

What, on the other hand, is the reasonable probability of things under compulsory notification law? The child shivers and sickens. Does the parent rush off to the doctor? Not at all. He at once recollects that the doctor, if he calls him in, will be compelled to advertise the infection to the civic sanitarians, and that he will at once become, thereby, liable to the attention of the sanitary inspectors and whitewashers, and possibly be obliged to submit to the removal of the child to hospital. Naturally he conceives that it will be quite time enough to face this annoyance when the disease has shown itself unmistakably; so he keeps the child quiet in bed, probably in the nursery with other children, for three or four days, until the rash has come out. Thus, the disease goes unchecked, the child's life is endangered, and the infection is very likely communicated to others of the family. But when there can be no longer any doubt as to the nature of the disease, does the parent *then* send for the doctor? No! the disease is, he thinks, at its worst, the child he supposes is going on nicely, and, in any case, he might now get into serious trouble if he said anything about the matter; so he does the best he can for himself—gets a bottle, perhaps, at the chemist's, and keeps the whole business a profound secret. And why has he run all this risk? Because the apprehension of the disease being reported by the physician has prevented his sending for medical help in the first instance, and he has thus been left without the advice and encouragement so needful under the circumstances.

I again appeal to the experience of the profession, and the common sense of the public, to confirm the accuracy of this picture, and of my statement that compulsory notification by the physician means concealment of disease, peril to the lives of the infected sick, and dissemination of the infection much more extensively

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than now takes place. I shall give proofs that the effect of imposing the notification upon the doctor, which we have here anticipated, has been realised in practice, and that in towns where such system is in force, exclusion of the physician, concealment of disease, and consequent increase of mortality are rife.

THE PENALTIES OF NOTIFICATION.

It is evident from the terms of the notification clauses inserted in the various private Acts that their promoters acted on the idea that the more stringent and penal the law could be made the more perfect would be the notification system, and I desire to point out that the direct contrary of this is the fact. It may be admitted that if the sequences of notification were less to be feared by the householder than the results of concealment, no disease would ever be kept secret; but the more tightly the sanitary line is drawn—the more thorough the Medical Officer of Health is in following up and stamping out the disease by disinfection, isolation, and removal to hospital, the greater is the inducement on the part of the householder to keep the disease hidden and to keep the physician away. For this reason it is important to see to what lengths sanitarians will go in their zeal for stamping out, forgetting that strong measures in this direction are immediately productive of the concealment to which we have referred.

Let us first take the law as it stands. The Public Health Act, sec. 137, declares, where the sanitary authority, in case it "is of opinion that the cleansing or disinfecting . . . . would tend to prevent or check infectious disease, it *shall be the duty* of such authority"—

a. To enforce disinfection.

b. To seize and destroy all bedding, clothing, &c.

c. To remove forcibly "any person who is suffering from any infectious disorder, and is without proper lodging or accommodation."

d. To punish with a fine of £5 anyone who exposes himself or any other person, if infected, in any public place whatsoever, or who "gives, sells, lends, transmits, or exposes" any infected clothes, bedding, or other material.

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e. To impose a fine of £20 on anyone who hires to anyone an infected house, or even a single room in such a house.

f. To punish every person who sends a child to school within three months of its contact with infectious disease, even if it has only resided in a house in which such disease has been.

This is the law as it stands, without the addition of compulsory notification, and it might reasonably be said to be fully sufficient for dealing with any outbreak of infection; and so it *would* be if due vigilance and care were exercised by the officers of sanitary authorities to trace the infected cases as they occur. But the foregoing enactments are the mildest rose-water remedies in comparison with the legal burthens which the compulsionists would put upon the community. Some of the vagaries of the sanitarians are truly astounding. At a recent meeting of the Social Science Congress, the seconder of a resolution in favour of compulsory notification eulogised a system which, he said, was in operation in some foreign land, under which the unhappy victim who happened to have a case of infectious disease in his house, found, the moment he had notified its existence, that the civic sanitarians pasted across his hall-door a yellow placard, which it was imprisonment for him to remove, and followed up the proceeding by putting a notice at the street entrance, warning all the world against entering therein. Another medical officer spoke rapturously of an American arrangement, by means of which—the instant the occurrence of a case of infection was known—sixty fac-simile advertisements of the full particulars were transmitted by next post to the sixty public schools within the bailiewick.

Those who are willing to stop short of this perfection of compulsory notification, will find provisions nearly as tyrannical in any of the Bills recently promoted for towns in England. To take an example, I find in Salford a Bill which gives the Corporation the following powers:—

(1.) The Corporation from time to time *may order* public or private *schools* or *any other place of public resort situate in the neighbourhoods affected by any infectious disease to be* temporarily *closed* or suspended.

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(2.) The Corporation may order any *shop, dairy,* building *or place for sale of beverages* by retail, *or for the sale or storage of provisions or of clothing,* or any common lodging house, *to be temporarily closed.*

(3.) *The Corporation may* declare any house or any part thereof to be an infected place and forthwith the following still more coercive regulations shall be observed:—

(*a*) *No person shall* in any such house or part of a house *exercise any in-door occupation* which necessitates the handling of any clothing food (including beverages) or article likely to communicate or retain infection;

(*b*) *No bedding, clothing* or *other articles shall be removed* from such house or part of a house without proper precautions *for the purpose of being disinfected or destroyed;*

(*d*) *No person dwelling in any such house or part of a house shall work at any other place in the same room with healthy persons* except with the written permission of the Medical Officer of Health, and after disinfection of the person and clothing;

(*e*) No nurse attending shall attend as nurse on any other person unless with the written permission of a registered medical practitioner and after disinfection of the person and clothing.

To suffer imprisonment for a breach of this monstrous code of laws one need not have been infected, nor even in contact with an infected person, and no amount of cleansing and disinfection of the person would entitle him or her to pursue his or her daily work.

I insist that law of this sort, if enforced (and if not enforced it is a farce), must make needful sanitation a terror to the victim of infectious disease, and must inevitably lead a large section of the public to conceal disease, and I contend that it is most unjust that the physician should be made, against his will, the agent for imposing these terrors on his patient.

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COMPULSORY NOTIFICATION A SHAM.

I am told that these laws are made to give power to the sanitary authority to deal with every emergency which canarise, but that they are intended to be used only under very exceptional circumstances. When I ask what are the exceptional circumstances which might justify the employment of this law, I am answered that it is only applied in case of the poor, and that the better classes are practically exempt from its operation. Indeed, in Edinburgh, Nottingham, and some other towns, this partial administration of notification law and its penalties is fully recognised, and it is left entirely to the discretion of the medical attendant whether sanitary precautions shall or shall not be put in force.

At the bottom of every notification paper is appended the words, "———— immediate attention is necessary," and the physician is invited to write the word "No" into the blank space in case he does not wish to trouble his patient with the interference of the sanitary authority. Now, what does all this mean? Is it not clear that here is one law for the poor and another for the rich; that wherever the physician fears to annoy his patient by invoking the intervention of the Medical Officer of Health, he writes in "No," and leaves the precautions against spread of infection to the voluntary action of his patient; that, on the other hand, wherever the patient is poor and friendless, the doctor is very likely to relieve himself of further trouble and responsibility by omitting the "No," and allowing the patient to be removed to hospital, or otherwise dealt with, as the sanitary authority may desire.

It must be obvious that, under these circumstances, notification is not compulsory but discretionary, and that the harbouring of infection is, amongst certain classes, the rule, and sanitation the exception. To quote the experience of towns in which such system exists, as evidence in favour of compulsion, is puerile, for such experience can prove nothing except that certain doctors have earned a considerable number of half-crowns by notifying cases which may or may not be infectious, the notification being, in a large number of cases, unproductive of any other result whatever.

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NOTIFICATION BY THE PHYSICIAN CANNOT BE ENFORCED.

I have undertaken to convince you that, however desirable notification of infective disease may be, it must always be a voluntary act on the part of the physician, and cannot possibly be enforced upon him by legal means. It is a fact that, if the physician does not please to notify, no power on earth can make him do so, and no Act of Parliament can punish him for omitting to do so. The fact will be realised when we remind our readers that the diagnosis of an infectious disease is a simple matter of professional opinion, and no man can be prosecuted or punished for making an incorrect diagnosis, or forming an inaccurate opinion.

A physician may be called to see a scarlatinal patient; he may recognise the disease, but he may not choose to annoy his patient by notifying, or perhaps he may intend to send the necessary notice to the sanitary authority but may neglect or forget to do so. Perhaps the patient dies, and the knowledge of the case comes directly to the Medical Officer of Health. How is the physician to be punished? It is true he may be summoned to the police-court and charged with an offence against the proposed law; but it is also true that no evidence can possibly be produced which can convict him.

In the first place, he may readily defeat the prosecution by availing himself of the phraseology of the law which says that the practitioner "attending, or called in, shall, on becoming aware" of the infective nature of the case, report it. He may plead that he was not "attending" the case, but simply happened to see it on the occasion of a friendly call; that he was not "called to visit" the patient, but to give a general opinion on sanitation; that he did not "become aware" of the nature of the disease, and so forth. But if defeated on these points, he has only to say that he did not recognise the disease as infectious, or boldly assert that it was a case of nettle rash, or some other non-infectious disease, and he *must* be discharged free. True, he may be called an ignoramus by the Medical Officer of Health for not recognising such obvious symptoms, but no further penalty can be inflicted on him, seeing

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that stupidity or ignorance of the symptoms of disease are not crimes punishable by law. Therefore, I say that the proposed law would be altogether impotent as regards the very class of practitioners whom it is intended to reach. Experience proves that it is true. In the seventeen towns in which notification by the physician has been in force for years past, not a single successful prosecution has been effected, though the omission to notify has been palpable in every one of them. In Jarrow one prosecution was tried, but it broke down for the reason which I have urged. In Edinburgh and in Bolton prosecutions have been successfully carried out, but on these occasions the recalcitrant physician had pleaded simply either that he did not know the law, or that he objected to it, neither of which pleas were, legally, of any avail. If he had excused himself on the ground that the case was not one of infective fever, or that he did not recognise it up to the date when he was summoned, or that he was not *in attendance* on the affected patient, he would, as a matter of course, have got off scot free.

So fully is this difficulty of enforcing the law recognised, that notification, though compulsory in theory, is purely voluntary in practice. I challenge denial of the statement that, as a rule, physicians practising in notification towns report cases which they can report without doing violence to their patient's wishes, and that they abstain from reporting cases which the patient's friends desire to conceal. In fact, the penal clauses of the Notification Acts are, and must always continue to be, a dead letter as long as they are directed against the physician, and based upon the erratic judgment of a practitioner, respecting the diagnosis of an obscure disease.

NOTIFICATION BY THE PHYSICIAN A PROTECTION TO QUACKS.

But, to my mind, one of the strongest objections to the proposed law is that its inevitable effect must be, and has been, to divert practice from conscientious and competent physicians to those who are ignorant and scheming, and to encourage by the strongest incentives the existing disposition of the lower-middle classes to resort for advice to counter-prescribing chemists, and

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other unqualified persons. The 4th section of Mr. Hastings' Bill declares that "*every medical practitioner* attending or called in to visit such inmate shall notify." It is thus only the legitimate practitioner who is subject to punishment for omission, the multitude of unqualified assistants, prescribing chemists, and quacks being free of any responsibility whatever. The proposed law does not in any way interfere with the counter-prescriber's practice. He is not amenable to it in any way. He may visit small-pox patients, treat them, and conceal the disease all through, and cannot be even asked to report, much less punished for omitting to do so. If, therefore, the custodian of any infected patient desires to escape the publicity and inconvenience attendant upon the notification of the case to the sanitary authority, he may at once escape all trouble of the sort by resorting to a chemist or quack practitioner. Thus, notification law, wherever enforced, has always increased unlicensed practice at the expense of legitimate medicine; and it must always have such effect as long as the physician and not the custodian of the patient is the person responsible to notify.

Should this pernicious law become general it is easy to see how the tide would flow. The conscientious physician will do his legal duty regardless of the feeling of his patient, and will suffer the consequent penalty of losing his patient, his money, and his reputation, whilst the practitioner whose purpose is to make a practice and conciliate his patient, will find all the excuses which the proposed law affords to abstain from notifying.

Let us not pretend to more virtue for medical men than for other classes of practitioners of similar social standing; and let us, therefore, not forget that a very numerous section of our profession will certainly not, if they can help it, do anything which will interfere with their own interests in practice. With such practitioners "Business is business." If they are earning daily fees by attendance on a case in private, they will scarcely be expected to put an end to their own profits by reporting the case, and thus causing it to be taken off to hospital.

Thus, in every town where compulsory notification has been enacted, the conscientious, timid practitioner has suffered seriously

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by his infectious cases being diverted into the hands of less honest persons; and it is evident that the inevitable effect of such law must be to handicap honesty in practice heavily, in its competition with the trade trickery which is an important element in the success of low-class practitioners.

CONCEALMENT OF INFECTIVE DISEASE.

I have endeavoured to show that the natural tendency and immediate result of stringent enforcement of sanitary precautions is concealment of disease, and that it is, with a view to secrecy, reasonably to be expected that the physician will be habitually excluded from the early treatment of infective cases if he be made the agent for putting these sanitary precautions in force. That my anticipation is justified by experience is capable of proof by the testimony of physicians engaged in the treatment of this class of disease. This testimony is almost the only sort of evidence at my disposal, because the vital statistics of these towns are practically available only to their health officers and civic authorities, and are not tabulated in any official publication. From these statistics—from a comparison of the number of deaths registered as "uncertified"—*i.e.*, having taken place without the attendance of a qualified practitioner—it would have been easy for the advocates of compulsory notification to prove that my anticipations are unwarranted. They have been repeatedly challenged to this test by myself and others, but have kept silence and declined our appeal to facts, and it may therefore be assumed that the figures, if available for public examination, would prove my case. The most complete series of proofs on this point is to be found in a *brochure*, published by Dr. Robert Hamilton, Senior Surgeon of the Liverpool Southern Hospital, and a member of the Town Council. This statement is compiled from replies which the Health Committee of the Town Council had received from the civic authorities of twenty-six towns, and also from the more reliable report of a deputation which had been appointed to investigate the subject, in person, in eight towns in which the compulsory notification system was in force. I recapitulate, as briefly as possible, the results of

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these inquiries in reference to concealment, as given by Dr. Hamilton :—

"BOLTON.—That concealment of disease occurs was unwillingly admitted by the Medical Officer of Health. This fact is, from its nature, a very difficult one to ascertain. But if there is one piece of information more than another which has been impressed upon me by my visit to the different towns, as an effect of the compulsory notification of disease, it is this of the concealment of the more prevalent diseases—measles, scarlet fever, and fevers generally, by the public. The slighter cases of all these diseases are kept secret by the parents or relatives, are unattended by medical men, are not surrounded by disinfectiug precautions, because of the 'tell-tale' nature of the latter; and so these slight cases became the 'nidus' from which numerous others, more severe ones, sprung.

"BLACKBURN.—The Medical Officer of Health did not think concealment occurred. Dr. Cort, on the other hand, had found it, especially among small shopkeepers and beerhouse-keepers, and Dr. Morley amongst people of a higher grade.

"WARRINGTON.—The inhabitants evade the provisions of the Act to a large extent. The sanitary authorities were compelled to admit this concealment of infectious disease.

"LEICESTER.—There is a class of practitioners, and that a very large one, in Leicester, who just treat the Acts as a dead letter, and send in no notifications; and the medical officer practically admitted all this. He had 'winked, and should think it advisable to wink, at non-notification in many cases.' In Leicester the concealment of disease was not disputed.

"NOTTINGHAM.—The opinion of an eminent medical man, Dr. Bell Taylor, is a very decided one. That the Act, since it has been put in operation, has not had the slightest effect in diminishing the death-rate, but that it has led to concealment of disease by the householder and by the medical attendants—that the latter do not notify, or if they do it is often weeks after; also that the operation of the Act has not facilitated isolation of infectious disease, but rather the contrary, because it had led to concealment, to which isolation would be antagonistic."

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From Glasgow, Dr. W. T. Gairdner, one of the most distinguished physicians in the kingdom, writes as follows:—

"In Glasgow, if not in Edinburgh, one of the greatest of evils is that in hundreds of cases of epidemic disease among the poor the medical practitioner is not called in at all, or only casually, and at a very late stage of the disease—in the very cases in which it is most urgent to discover such diseases early—the cases in which there is the greatest ignorance or the most deliberate intention to hush up the facts. No doubt the machinery works smoothly enough as long as the householder and the doctor together are agreed as to the public duty of notification. But I have been long enough a sanitary medical officer to know that these are not the cases that most require looking after."

Dr. Carter, of Liverpool, writing on the same subject, says:—

"We shall be told that to argue from the inevitable tendencies of human nature would be to show ourselves theorists, that your practical men must have facts. To facts, therefore, we will turn; and, difficult though in the nature of things it must be to prove concealment, I think that the evidence that it does exist is overwhelming.

"Firstly, let me ask your consideration of a few figures, taken from such reports of Medical Officers of Health as have come to my hand. In Blackburn, during 1881, there were 103 notifications of scarlet fever, and 23 deaths, or 1 in every 4. In Bolton, during the fifteen months ending December 31st, 1880, there were 702 cases reported, and 112 deaths, or 1 in every 6·2. Or, turning to another disease—in Bolton, during the same period of fifteen months, there were 102 notifications of typhoid fever, and 23 deaths, or 1 in every 4·4. In Blackburn, during 1881, there were 281 notifications, and 68 deaths, or 1 in every 4·2. Now, I would ask any medical man, whatever his practice may be—even if it be among the very lowest and poorest of the land—if he has ever known, in the most serious and fatal epidemic, such ghastly mortalities as these? The deaths from typhoid fever in Blackburn and Bolton were half as high again as at the London Fever Hospital, where, for reasons given by the late Dr. Murchison, they

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must always be exceptionally high. The only conclusion that seems possible from such figures as these is, either that many cases are concealed, and hence the mortality is made to appear high; or that the diseases themselves, if at all of a severe type, are fearfully fatal to the individual, where such powers as those possessed by Bolton are carried out."

HAS COMPULSORY NOTIFICATION REDUCED THE DEATH-RATE.

Since Dr. Carter wrote thus, the only comprehensive statistics on the notification question have been set forth in the "Manchester Health Journal," published by Heywood, under the editorship of Dr. Scott, and these valuable tables are compiled from reports obtained voluntarily from twenty-two notification towns, and their mortality returns cover the three years last past. They contain, however, only four quarterly returns of the number of "uncertified" deaths, but, so far as this information goes, it shows, on average of the twenty-two notification towns, *an increase* of the number of such deaths from 4·0 to 4·6 per cent. of the total deaths.

Again, I find that in Dublin, although no law exists to compel the physician to notify, the effect of such a law is foreshadowed by the result of a limited notification. The Dispensary District Medical Officers are the *ex-officio* District Medical Officers of Health and the servants of the Town Council, and, as such, they are under orders to report infectious cases to the Public Health Committee. They have done so, and the Corporation disinfectors have, thereupon, been sent to adopt the necessary sanitary precautions, and have been obliged to do so with every consideration for the feelings and the interests of the patient's family, inasmuch as no special powers exist to effect sanitation or isolation by force. Nevertheless, it is notorious that the result of this very limited system of notification in Dublin has been to exclude the Dispensary District Medical Officers from practice amongst the poorer classes, and to throw the treatment of infectious cases into the hands of "club" doctors, who, not being corporate officers, are not obliged to notify, and, in fact, do not do so. The infected poor of Dublin, in many instances, even deny themselves the necessaries of life in order to be able to

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pay a private practitioner, rather than resort to the dispensary doctors, and the Public Health Committee of the Corporation have publicly stated this fact in their official reports. They say:—

"The attention of the Public Health Committee was drawn to the fact of concealment of infectious disease, and the serious consequences of neglecting to call in medical aid until the illness is of several days' duration. The fear of incurring the censure of the landlords of tenement houses in many instances appears to be the chief reason for not seeking medical advice in time; with others the dread of removal to hospital is the motive."

THE TEST OF THE VALUE OF NOTIFICATION.

I say, advisedly, that in towns in which compulsory notification is the rule, concealment does take place to a very serious extent, and I ask the advocates of compulsory notification by the physician—Can you prove by statistics of the number of persons who died *unattended by a qualified medical practitioner*, within towns in which notification is compulsory on the medical attendant, that the system has not caused concealment of disease, or driven the population into the arms of quacks and prescribing chemists?

There should not be the least difficulty in giving definite replies to this query, and without such replies no one can judge of the value of compulsory notification.

Under the law of Death Registration every information desired is available, and that it is, therefore, easy for a Medical Officer of Health to compare, day by day and year by year, the mortality from any class of disease, and to illustrate the actual results from notification by showing that zymotic deaths have been reduced in number by its agency, and to what extent persons dying from infectious disease have been attended by quacks or prescribing chemists, or left without attendance.

One only out of the twenty-two Medical Officers of Health (Huddersfield) has made any attempt to produce this evidence; they have given abundant proof of a reduction *in the number of cases of infectious disease reported* under the notification system, and they have most illogically argued that because reports have become

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fewer since the compulsory notification system was adopted, disease must also be less prevalent. I say that a falling off in the number of reported cases proves the exact reverse; that it shows that the public and the medical profession who, at first, sent in their notifications because they knew nothing of the consequences, and feared punishment if they omitted to do so, have since learned that punishment is improbable, and consequently disease is kept secret.

It is easy to evade notification—impossible to cheat death—and therefore, in my opinion, a comparison of *death-rates* before and after the introduction of the notification system would have been far more reliable as a test of improved health than any comparison of semi-voluntary notification reports.

This form of evidence is, unfortunately, not at my disposal. I have addressed to the English Registration Department a request for the information, and I have been answered that no local mortality statistics are to be had except in the reports of the local authorities.

A report laid by the English Local Government Board before the Select Committee gives us, however, some information. In the first place, it proves to us beyond doubt that, in many towns in which all sorts of benefits are alleged to have resulted from compulsory notification, the compulsory notification system has really no existence, and yet the supposed gain to the public health is as palpable as in those towns in which the system is rigidly enforced.

Comparing the number of cases reported in individual towns with the population of those towns, we find that in Rotherham the proportion was 3·4 cases per cent. of the population; in Preston and Leicester, 1·5 per cent.; while in Stafford, the percentage was only 0·075; and in Huddersfield, only 0·22. In other words, notification was forty-five times less active in Stafford than in Rotherham, yet I find it officially stated that the local authorities were just as well "satisfied" in the former as in the latter town. Again, in Manchester and Huddersfield, the local authorities are, as they say, thoroughly "satisfied" with the working of the system, although, in these towns, the Acts do not produce one report for every half dozen which are sent in in other towns. It must, in fact,

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be taken as proved by this table, that, though the diversities and the variations in the extent to which notification is in activity in different towns, make the system an absurdity, nevertheless all the Medical Officers of Health are equally "satisfied" with their own method of using the law, and the public health of all the towns seems to be equally benefited. The conclusion is quite enough to discredit the whole of the reports upon which the Select Committee formed its judgment.

ANALOGY OF DEATH REGISTRATION.

But it has been argued that, as concealment of causes of death has not resulted from compulsory death registration, so concealment of disease does not necessarily follow compulsory notification. I submit that there is very slight analogy between the two cases, because the possible inconvenience to the patient's family, arising from death registration, are not in any way comparable with the actual and serious *desagremens* following upon disease notification. But the true facts of death-certifying prove conclusively my case against the proposal that physicians should be compelled to notify infectious disease, for every one knows that death-certificates are, in many cases, utterly fallacious and misleading, and are so because it is desired to conceal the true cause of death. Concealment, such as I anticipate from physician-notification, is, in fact, practised to a great extent in death-registration, wherever there is an incentive to concealment. If anyone were to estimate the prevalence of *syphilis* or of *delirium tremens* from the Registrar-General's returns, he should at once conclude either that those are almost unknown diseases, or that they are seldom or never fatal, for we rarely find a death certified under either of these names, but we find an immense number of cases of death recorded as resulting from "spinal irritation," "urinary disease," "debility," and such like euphemisms for these obnoxious diseases, and we also find a vast number of deaths registered as resulting—not from the disease which actually killed the patient, but from the symptom which was most prominent at the time of death. As a matter of fact, syphilis or *delirium tremens* as causes of death are concealed whenever the family of the

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deceased would be likely to be scandalised by the suspicion that such disease existed amongst their relatives, and are reported only in the cases of paupers and other persons respecting whom there is no reason for secrecy. As an illustration of the prevalence of this practice, I point to the Annual Report of the Registrar-General for Ireland, for the year 1882, from which it appears that in a population of 5,145,000 persons, there were registered only 125 deaths from syphilis in persons over 20 years of age; and 13 from gonorrhœal complications—only 36 from *delirium tremens*, and 114 from chronic alcoholism.

FAILURE OF THE NOTIFICATION SYSTEM.

I have undertaken to produce evidence that, as a matter of fact, there is no sufficient proof that compulsory notification has produced or promises to produce any beneficial effect whatever on the public health.

It is indeed the sole argument of the compulsionists that compulsory notification by the physician, whatever its faults may be in principle and theory, has been found, in practice, to be effective and convenient, and ought, therefore, to be the universal law; they appeal to experience against every objection which is raised. Let us see what answer experience affords, and let us satisfy ourselves whether there is, indeed, any conclusive proof that compulsory notification by the physician has produced, in the communities in which it has had a full trial, any benefits commensurate with the personal annoyance and public expenditure which it has involved. It is emphatically my contention that, speaking generally, no such benefits have resulted; that the data upon which medical officers of health argue out the case for compulsion are altogether unreliable, and prove nothing; that the true tests for an improvement in the public health of notification towns have been ignored, or purposely kept out of sight. I boldly assert that, as a matter of fact, zymotic mortality has been rather increased than diminished, and the danger of infection rather been aggravated than mitigated by the introduction of the law of compulsory notification by the physician.

I am justified in asking the advocates of compulsion why it is

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that there should be any doubt on this point, and why it is that they are unable to prove the efficacy of their great sanitary panacea by incontrovertible statistics? They have had the uncontrolled use of notification compulsion for eight years in Bolton and Greenock; for seven years in Burton, Jarrow, and Nottingham; for six years in Blackburn, Blackpool, Derby, Leicester, Llandudno, Norwich, Rotherham, Warrington, and Edinburgh; and for shorter periods in eleven other English towns; and they surely ought to be able to point to the vital statistics of these towns as unquestionable evidence of the benefits of physician-notification, if such benefits be demonstrable. I assert that the compulsionist party, speaking through the mouths of the Medical Officers of Health of these towns, have utterly failed to adduce any such proof.

At the suggestion of Sir William Harcourt's Special Committee on Private Bills, the English Local Government Board obtained from the Medical Officers of Health of all notification towns, a series of reports which were subsequently printed as a Parliamentary paper. These reports are the only available source of information open to me. On them the whole case of the advocates of physician-notification is based, and I assert that they omit carefully the proofs which, in favour of compulsory notification, might carry conviction to the public mind, and contain nothing whatever to satisfy us that it is desirable to make general the law which has heretofore been local. The questions put by the Local Government Board were addressed to the Town Clerks of notification towns, and to no one else; the medical practitioners in those towns, who know more, probably, of the actual working of the Acts than the corporate officials, were not asked a question; nor was it known to them that the Committee was about to sit, or that inquiries were about being made in which they were, both professionally and personally, deeply interested.

The questions put to the Town Clerks by the Local Government Board would also seem to have been carefully worded, for the purpose of leading the Medical Officer of Health to give a reply favourable to compulsory notification, and to conceal all facts which might throw doubts on the efficacy of that system.

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They were as follows:—

"*a.* Are the local authorities satisfied with the working of compulsory notification?

"*b.* Have they any suggestions to make on the subject?

"*c.* Was public attention specially directed to the intention of the local authority to apply for the (notification) powers?"

Of course the local authorities are satisfied. Why should they not be, considering that compulsory notification by the physician supplies them with an unpaid corps of sanitary detectives, who, if they do not work to their satisfaction, may be fined and imprisoned? Of course the Medical Officer of Health is well "satisfied," considering that almost all the trouble of detecting infection, of following it up to its sources and punishing its disseminators, is taken off his hands by the forced labour of the local medical practitioners, and he is thus relieved of a most onerous duty.

In forming a judgment upon the valuation of the notification system based upon the mortality statistics of towns, it would be necessary to compare—first, a period as long as possible before compulsory notification was introduced in the town, with a similar period subsequently; second, to compare the mortality of notification towns with that of others of equal population, and similarly situated; thirdly, to make allowance for the influence of the Public Health Act, and other sanitary influences, in calculating the causes of the improvement of health. This has not been done by any of the advocates of notification who have generally contented themselves with pointing out the decrease *in cases notified*, and triumphantly claiming that these represented an actual improvement in the public health.

After what I have said, it will be readily understood that I have no faith in any conclusion founded upon the number of notifications, nor upon the zymotic death-rate, believing, as I do, that the notifications do not necessarily represent zymotic disease, and that the deaths from concealed zymotic disease are not in very many instances truthfully recorded.

Dr. Carter, of Liverpool, has, however, produced some cogent figures bearing on the subject.

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"One would think (he says) that, so far as fairness could be obtained at all by a method in itself radically defective, it would have been by a comparison of the sanitary condition of a town during an equal period before and after compulsory powers were obtained. But Bolton compares ten years before with only five years after; Huddersfield, eight years before with five years after; while Warrington carries the absurdity to its furthest limits by comparing nineteen years before with two years after. It should be specially noted that even the shortest of these periods carries us back to a date some years antecedent to the Public Health Act of 1872, before which, as everyone who has considered this question at all knows, decade after decade passed without the slightest sanitary progress throughout the country.

"*Average Annual Mortality per* 1,000 *Inhabitants.*

	Bolton		Liverpool	
	1867–76 (10 years before the Act)	1877–81	1867–76	1877–81
Small-pox	0·12	0·004	0·54	0·12
Scarlet fever	1·10	0·82	1·45	1·04
Fevers (typhus, typhoid, simple)	0·57	0·33	1·15	0·57
Populations	94,000	105,000	493,000	535,000

"Yet, accepting what must be considered as the most favourable statement possible for these towns, that which they and others specially put forward as proving their case, what do we find? Why, that Liverpool, which for many reasons is more liable to the inroad and spread of infectious disease than any other town in the kingdom, has progressed during these very years nearly half as fast again without any compulsory Act, as Bolton has with one. If you will examine the figures on the table you will see that the average annual mortality per 1,000 from small-pox, scarlet, typhus, typhoid, and simple fevers, fell, in Bolton, from 1·79 in the first period of ten years, to 1·54 during the second period of five years; whereas in Liverpool, during the same periods, from the same diseases, it fell from 3·14 to 1·73. If

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Bolton had progressed as fast as Liverpool, her mortality from these diseases should have been 98 per 1,000 instead of 1,154. Yet the Medical Officer of Health, to quote his own words, considers that "the above evidence illustrates in a striking manner the beneficial effect of the compulsory clause, and fully justifies all that has been said in its favour." And if, instead of comparing Bolton's progress in these diseases with that of Liverpool, we compare it with that of the country generally, we find a similar result—viz., Bolton, with her compulsory notification, lagging considerably behind the country generally without it. The average annual mortality per 1,000 for England from these diseases during the earlier period was 1·78. Bolton and the country then started on the race for improvement from almost exactly the same level. Yet, during the succeeding four years (the only ones for which the Registrar-General's returns admit of a comparison), the country generally had progressed so much the faster, that its average annual mortality from them had sunk to 1,047 instead of only to 1,154.

"In the same way, without going into details, it can be shown that, if Liverpool had only progressed as fast as Huddersfield, in these test diseases, her mortality from them during the latter period should have been 1·8 per 1,000, instead of, what as a matter of fact it is, 1·73."

The returns published by the "Manchester Health Journal" has enabled me to compare the total death-rate of 22 notification towns, representing a population of 2,346,000, for three successive years—1882-3-4. I have taken the total death-rate—firstly, because it gives me a longer period for comparison; secondly, because it is independent of the errors of nomenclature incidental to the reporting under the notification system. I find that the 1882 average rate for 16 towns was 22·8 per 1,000 of population; the 1883 rate 21·4; the 1884 rate 22·3; so that, so far as three years of notification can prove anything, no progress in public sanitation has been made by the operation of this much-vaunted law.

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THE PHYSICIAN'S VIEW OF THE NOTIFICATION SYSTEM.

It will be observed that, in discussion of the question, I have purposely abstained from presenting it, as I might, with every reason, have done, from the point of view of the physician who is forced to render public services without any adequate remuneration, and in direct violation not only of his inclination but of his interests and of his relations towards his patient. Lest it may be supposed that I have allowed judgment to go by default against our profession, or have acquiesced in the imposition of compulsory notification upon us, I must take occasion to repudiate with my utmost emphasis and earnestness the right of the community—for any purpose, however valuable or laudable—to seize upon my professional knowledge, experience, and labour, unless it pleases me to render them for the public use.

The advocates of compulsory notification have argued that the physician has nought to grumble at if he is forced to do public work against his will, and for an insignificant fee which he does not desire to earn, because, forsooth, he enjoys a monopoly of medical practice granted by the State, and is, therefore, morally liable to do the bidding of the State in all matters within his scope. I shall not stop to prove that the State does not confer upon the physician any monopoly of medical practice, but I shall at once take issue with such an argument on plain principles of justice and custom. Would the physician be under any compliment to the State if he were permitted to practise his profession to the exclusion of other people? Does he not repay the public for any privileges granted to him, not only by the expenditure of his time, money, and brains, on special education to fit himself to enjoy the monopoly, but by cash paid for leave to receive those privileges, and by incessant, unrequited work for the public benefit?

Surely the balance of obligation is overwhelmingly in favour of the medical profession when State monopoly and professional public work come to be placed one against the other; and if so, by what right, save that of overwhelming necessity for the welfare of the community, can the public claim to burthen any individual physician

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with any additional duty whatever which he is unwilling to perform? Are not pharmacists, solicitors, barristers, clergymen of the State Church, even cabmen, all monopolists as regards the practice of their profession? and yet no one talks of forcing them to do public work with or without adequate compensation. The remuneration of a shilling or even half-a-crown, payable to the physician for thirty days' notification of disease in any one house, can scarcely be considered adequate; but, if it were, the objections of the medical profession would remain the same. Physicians protest against being forced to do public work and sell professional confidence no matter what the price may be.

Time does not permit me to enter upon a *résumé* of the method by which these Acts have been smuggled through Parliament—of the use which has been made of the English Local Government Board to secure official approval for the system, and of the fact that the Select Committee, by a majority of one, refused either to hear evidence upon the part of the profession or to make any reasonable inquiry as to the working of the notification Acts from anyone, save those interested in carrying those Acts through an uninformed and uncaring Parliament.

I conclude this communication with the heartfelt regret that in this, as in most other legislative affairs—in which the public, in medical matters, and our profession, in its own interests, are concerned—we, the medical profession, find ourselves without any voice in the councils of the nation save that which can be expressed by one or two Members of Parliament, who are, of necessity, politicians first and doctors afterwards.

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SUB-SECTION OF ANATOMY AND PHYSIOLOGY.

NOTE ON A READY METHOD OF DEMONSTRATING THE ALTERNATION OF THE SECTORS OF THE CRYSTALLINE LENS.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; Professor of Surgery, Trinity College.

[Read in the Sub-Section of Anatomy and Physiology, February 19, 1885.]

Everyone who has experience of the teaching of practical anatomy will admit the importance of facile methods of demonstration of the structure of organs *in situ*. Such, I think, is this mode of showing simultaneously the lines, sectors, or septa, as they are variously named, which exist on the surfaces of the crystalline lens, and so rendering evident the fact that each sector bisects the angle formed by the junction of two on the opposite surface. This fact can, of course, be seen by careful inspection of the crystalline lens placed in a test tube or otherwise suitably fixed in water after removal from the eye, so that it can be held between the observer and the light, but a demonstration *in situ* far more completely fixes the student's attention.

In attempting the injection of the canal of Petit with mercury it happened, as I worked in a room lit only from the ceiling, that the metal escaped behind the lens, and formed a concave mirror in contact with its posterior surface. I then, thanks to the accident of my position under a pure head light, noticed the very complete exhibition of the sectors on each surface of the body. Repeating the experiment, I found that not uncommonly I failed to inject the metal into the space immediately behind the lens, and that it escaped to the deep surface of the vitreous humour. Here a larger

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mirror formed, which gave even a better means of illuminating the surfaces of the crystalline, and, of course, of exhibiting its sectors.

In making these experiments, if the eyes operated on be very fresh, they must be allowed to lie, for a short time after the dissection of the posterior chamber of the aqueous humour, in water, else the sectors on either surface will be hardly visible; and again, if the eyes are stale, only the posterior mirror can be formed, for the mercury escapes at once to the fundus.

It must be borne in mind that only under a vertical illumination can the demonstration be made effectively to a class or even to one's own observation, and that day-light is almost essential for class demonstration.

My apology for bringing so trivial a matter before the Sub-Section is that I found its advantage during many years of practical teaching of anatomy, and that since I have retired from this field I have not known of any teacher availing himself of its aid.

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ANOMALY OF THE AORTA.

By J. S. M'ARDLE, F.R.C.S.;
Surgeon to St. Vincent's Hospital.

[Read in the Sub-Section of Anatomy and Physiology, February 19, 1885.]

In the specimen which I have exhibited the transverse portion of the aorta passes almost directly backwards to the left side of the third dorsal vertebra; thence the vessel passes to the neck of the seventh rib, from which point it returns to the middle of vertebral column. Below the origin of the renals it diminishes in size, becoming as small as a common iliac. The branches coming off from its convexity, from right to left, are—right carotid, left carotid, left vertebral, and left subclavian; from its posterior aspect at the junction of the transverse and descending portions the right subclavian arises.

The course of some of the branches is worthy of note. The right carotid passes upwards for about half an inch, lying in front and to the left of the trachea; it then crosses to the right, in front of trachea, œsophagus, right subclavian, and vertebral arteries, and at the level of the seventh cervical transverse process it turns upwards in its normal course. The left vertebral, from its origin between carotid and subclavian, passes backwards to the second intercostal space, upwards in front of neck of first rib. The right subclavian comes off from the posterior aspect of the aorta, at the level at which the remains of the ductus arteriosus joins it, runs from left to right in front of third and second dorsal vertebræ, then upwards on side of first dorsal to reach its normal position between the scaleni. In front of it are trachea, œsophagus, thoracic duct, right carotid, and transverse portion of aortic arch. All its branches come off from the first stage, the superior intercostal and profunda cervicis arising separately. At its origin the

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right subclavian is more than half the size of the aorta; it gradually diminishes, and before giving off any branch becomes normal in size. The thoracic duct passes for a short distance along the right subclavian, and then between it and the œsophagus.

Although the abnormalities which I have noted above are not infrequent as regards the number of branches derived from the arch, there are a few peculiarities in the specimen to which I would wish to call attention:—

1. The five branches may be said to come off from the transverse portion of the arch, and in any specimens which have come under my notice the fifth branch usually came off at the level of the fourth or fifth dorsal vertebra; in this case it arises at level of third.

2. In most cases in which right subclavian comes from back of arch it bears no close relation to right carotid; here they embrace the trachea and œsophagus between them, and in this situation two rings of the trachea are flattened out, and the tube considerably compressed.

3. The cone-shaped origin of the subclavian is not common.

4. The right carotid is more tortuous than is usual in such abnormalities.

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ON THE MUSCULUS STERNALIS OCCURRING IN ANENCEPHALOUS MONSTERS.

By FRANCIS J. SHEPHERD, M.D. C.M.;
Professor of Anatomy in M'Gill University, Montreal.

[Communicated to the Sub-Section of Anatomy and Physiology, by Professor Cunningham, of Trinity College, Dublin, February 19, 1885.]

At the meeting of the British Association held last summer in Montreal, Prof. D. J. Cunningham, of Dublin, read a paper on the "Value of Nerve-supply in the Determination of Muscular Anomalies," in which he stated his belief that the musculus sternalis belonged to the pectoral group—in fact, was an aberrant portion of the pectoralis major muscle, as suggested by Mr. Abraham, of Dublin. He gave as a reason that lately he had traced the nerve-supply of the musculus sternalis in five cases, and found that it came from the internal anterior thoracic nerve. Prof. Cunningham also threw out the suggestion that this might possibly be a new inspiratory muscle antagonistic to the triangularis sterni, as it acted, when well developed, as an elevator of the ribs. He also stated in this connection that although statistics on this subject were very meagre, he was of opinion that it occurred more frequently in females; and this, he thought, had a significance, as in the female costal respiration was always more pronounced than in the male. In the discussion which followed both Dr. Dobson and I held that it was most likely a remnant of the *panniculus carnosus* muscle. Dr. Dobson considered that the sterno-cuticularis muscle of the hedgehog closely corresponded to the m. sternalis. Prof. Cunningham also mentioned in his paper that Mr. Abraham,[a] of Dublin, had lately found that the m. sternalis occurred very commonly in anencephalous monsters, as he had discovered it in six out of eleven examined.

Since the meeting of the British Association I have examined

[a] Transactions of the Academy of Medicine in Ireland, 1883.

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six anencephalous monsters which are in the museum of the Medical School of M'Gill University, and have found in each one a well-marked example of the m. sternalis. My recent dissections of these muscles have had the effect of changing my previous views in regard to the homology of this muscle. I have become convinced that it certainly does not belong to the panniculus group, but very probably should be classed with the pectoral group, and for the following reasons:—(1) In seven out of the nine muscles found in these monsters (three having double muscles) the nerve-supply was furnished by the anterior thoracic, one of the seven, however, also receiving a branch from the intercostal as well. I was unable satisfactorily to make out the nerve-supply of two muscles, both of which occurred in the same fœtus. My belief is that it came from the anterior internal thoracic. (2) In three, the fibres of the abnormal muscle were continuous with those of the great pectoral (Figs. 1, 2, 6), and in one (Fig. 5) the fibres pierced the great pectoral. In others, portions of the great pectoral arose in common with the m. sternalis. (3) In several a portion of the m. sternalis was covered by the great pectoral. (4) The great pectoral was deficient on the side on which the m. sternalis was present in eight instances (Figs. 1, 2, 3, 5, 6). (5) In one (Fig. 6) the right platysma myoides was well developed, and passed some distance below the clavicle; it was separated from the m. sternalis of that side by a third layer of fat, and was in a plane quite superficial to the m. sternalis.

In all the cases except one (Fig. 3) the abnormal muscle was large and well developed, and had an attachment to the costal cartilages. The majority of the muscles were triangular in shape, though some were fusiform. In the last three dissected I had no difficulty in tracing the nerve-supply, as the nerve was always found passing along the interval where the pectoralis major was absent; thence over the pectoralis minor and through the costo-coracoid membrane to the internal anterior thoracic. The muscle was always supplied from its deep surface. In three of the subjects the muscles were continuous with the sternal insertion of the sterno-mastoid (Figs. 1, 2, 4).

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In two a portion of the muscle blended with the aponeurosis of the external abdominal oblique.

I do not propose in this paper to discuss the various views that have been held in regard to the homology of this muscle, as that has already been ably done by Prof. Turner;[a] but since Prof. Turner's paper was written Prof. Bardeleben[b] has advanced the theory that some of these muscles belong to the sterno-mastoid and are supplied by the intercostal nerves, whilst others he classes with the pectoral group, because they receive their nerve-supply from the anterior thoracic. Malbrane[c] agrees with Bardeleben, for in two living subjects he found the m. sternalis standing out quite perceptibly under the skin. In the first case faradisation of the intercostal nerves brought the muscle into action, but in the second it failed; but when faradisation of the thoracic nerves was employed the muscle responded immediately. (In Case III. in my series, the muscle was supplied by both the intercostal and the thoracic.)

M. Testut[d] holds that the m. sternalis (présternal) is in its upper part an appanage of the sterno-mastoid, whilst in its lower part it belongs to the external abdominal oblique. He asserts that these muscles are in the same muscular plane, and that the m. sternalis is the remnant of the old connection which existed between the two, as is seen in serpents. I am not now prepared to say what the morphological significance of the musculus sternalis is—whether it is a new inspiratory muscle appearing, as is held by Prof. Cunningham, or is a muscle *sui generis*, as Halbertsma thought, or a remnant of a previous connection between the sterno-mastoid and the external abdominal oblique, as suggested by M. Testut, though the nerve-supply would be against the latter theory. All I can now assert is that I believe the m. sternalis should be classed with the pectoral group, and is no doubt, when well developed, an elevator of the ribs. I wait for further light to *fully understand its significance.*

The fact that this muscle occurs so commonly in anencephalous

[a] Jour. of Anat. and Phys. Vol. I., p. 246.
[b] Ibid.
[c] Quoted by Testut in Les Anomalies Musculaires chez l'Homme, 1884.
[d] Anomalies Musculaires chez l'Homme. P. 84.

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monsters seems to me to indicate that the muscle should be regarded as a rudiment rather than a new muscle appearing in man. Five out of the six monsters dissected were females; the m. sternalis was found in all, and quite as well developed in the single male as any of the five females. As far as I can learn, anencephalous monsters are more commonly female than male.

I shall now describe the cases in detail:—

Case I. (Fig. 1).—*A well-developed Female Anencephalic Fœtus of full term; M. Sternalis well-marked and Unilateral.*—It arises from the fascia over the first piece of the sternum by a flat tendon, which is continuous above with the sternal origins of the right and left sterno-mastoid muscles, and on the right side is connected with the muscular fibres of the pectoralis major arising from the manubrium. From this origin the muscle passes downwards and outwards to the left side, expanding as it descends into a large fusiform muscle which is inserted into the whole of the fourth left costal cartilage and into the side of the sternum opposite the fifth and sixth costal cartilages; the innermost portion of the muscle is prolonged downwards over the lower part of the great pectoral and ends in the aponeurosis of the external abdominal oblique.

The muscle lies on the sternum and costal cartilages, and has only a few of the fibres of the pectoralis major beneath it. Above, on the outer edge, some muscular fibres come off from the m. sternalis and blend with those of the great pectoral.

The nerve supplying this abnormal muscle enters it about its centre, and is distributed to its deep surface; it can be traced back through a cellular interval between the fibres of the great pectoral, then over the pectoralis minor, at the upper border of which it pierces the costo-coracoid membrane and joins the internal anterior thoracic nerve. As it lies between the two pectoral muscles it gives off a branch, which goes to the lower part of the great pectoral.

Case II (Fig. 2).—*Female Fœtus, Anencephalous, and with a Spina Bifida (Cervical); Double M. Sternalis.*—The two muscles have a common origin from the first piece of the sternum, which is continuous above with the sternal origin of the sterno-mastoid of each side. The left muscle, which is much smaller than the right, flat and narrow, arises from the common origin, passes down to be inserted into the third left costal cartilage and side of second piece

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Fig. 1.

Fig. 2.

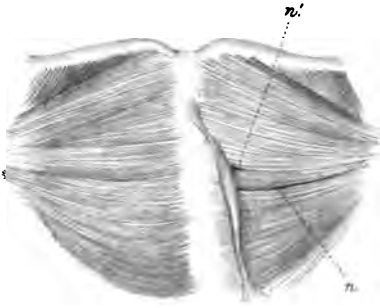

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

F Huth, Lith. Edin.

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of the sternum; at its insertion it is covered by the fibres of the lower segment of the pectoralis major.

The *right* muscle is a large, flat muscle, arising from the common origin, and also from the sternum opposite the second and third costal cartilages. It divides into three sets of fibres—the outer inserted into the upper edge of the lower segment of the great pectoral, the middle continuous with the fibres of that muscle, and the innermost inserted into the lower end of the sternum and upper part of the ensiform cartilage. On both sides a triangular portion of the great pectoral is absent; the space thus left is partly covered by the abnormal muscles; this space is larger on the left than the right side. In this fœtus, owing to its very friable condition, I was unable to satisfactorily trace the nerve-supply of the two anomalous muscles, but I am inclined to believe that they were supplied by the anterior thoracic, as I traced on each side a branch from the internal anterior thoracic nerve, over the pectoralis minor muscle, to the triangular interval between the two segments of the pectoralis major, but here I lost it.

Case III. (Fig. 3).—*Female Fœtus, Brainless, and with a Bifid Cervical Spine; M. Sternalis Unilateral, Left.*—In this case the abnormal muscle consists of a small fusiform slip, which arises from the sternum opposite the second left costal cartilage by a thin aponeurosis, passes over the great pectoral muscle, and spreads out into a broad aponeurosis, which blends with the fascia over the external abdominal oblique. It receives its nerve-supply from two sources—the larger branch enters the muscular part of the slip about its centre, and can be traced through the great pectoral, over the lesser pectoral, through the costo-coracoid membrane to the internal anterior thoracic nerve; the smaller branch enters the upper part of the muscle, and can be traced through the intercostal space to the third intercostal nerve; both nerves supply the muscle from its deep surface. This was the only case where the intercostal nerves gave a branch to the m. sternalis; in some of the others the muscle was pierced, but not supplied, by the intercostal nerves.

Case IV. (Fig. 4).—*Male Fœtus, Anencephalic, and with Bifid Cervical Spine; Muscle Unilateral, Left.*—The muscle in this case is of good size, flat and triangular, arising above from the manubrium sterni in common with the upper sternal portion of the right pectoralis major, and also with the sternal origin of the left sterno-

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mastoid, with which its left border is continuous. It arises by a flat tendon, and, as it passes down, it soon expands into a broad muscle, which is inserted into the whole of the third costal cartilage, being covered at its insertion by the pectoralis major. Its inner edge is prolonged downwards over the the lower portion of the pectoralis major, and is soon lost in its fascia. In the left side a triangular portion of the pectoralis major muscle, which arises from the upper part of the sternum and costal cartilages of the second and third ribs, is wanting, the space left being partly covered by the m. sternalis. The nerve supplying the abnormal muscle reaches it through the triangular interval between the upper and lower portions of the great pectoral; it can be traced beneath the great pectoral, and over the lesser pectoral to its upper border, where it reaches the internal anterior thoracic nerve.

Case V. (Fig. 5).—*Female Anencephalic Fœtus; no Spina Bifida; Muscle Bilateral.*—Both muscles arise, in common with the upper sternal fibres of the pectoralis major muscles, from the manubrium sterni, and diverge from each other as they descend. The *left* muscle passes down, and soon develops into a flat triangular muscle, which is inserted into the fourth costal cartilage near the sternum, and beneath the pectoralis major. Continuous with its lower fibres, and running along its inner edge, is a small muscular slip, which is attached above by a round tendon to the middle of the sternum, passes over the lower part of the great pectoral, and is inserted into the fascia covering it. A large part of the central portion of the great pectoral on this side is wanting, the space left being, as in the other cases, partly covered by the abnormal muscle. The nerve supplying the m. sternalis of this side reaches it about its middle, and can be traced, as in the other cases, to the internal anterior thoracic. The *right* muscle passes down from the common origin, and soon divides into two slips, the *outer* of which, after piercing some fibres of the great pectoral, is lost in the fascia of that muscle. The *inner* slip continues down immediately outside the sternum, and ends by a tendinous expansion which is lost in the aponeurosis covering the lower part of the pectoral muscle. On this side, also, a triangular portion of the great pectoral covering the second and third costal cartilages is absent. The nerve can be traced, as in the other cases, crossing the triangular interval, and joining the internal anterior thoracic nerve above the pectoralis minor muscle.

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Case VI. (Fig. 6).—*Female Anencephalous Monster, with Spina Bifida of Cervical and Dorsal Region; Muscle Bilateral.*—The muscles of the two sides have a common origin from the manubrium sterni. The *right* muscle, which is the larger, and triangular in shape, passes down over the triangular interval caused by absence of a portion of the great pectoral, and is inserted, by muscular fibres, into the upper border of the lower segment of the pectoralis major, and, also, into the sternum opposite the fourth costal cartilage; some of its fibres pass over the pectoral muscle and blend with it. As in the other cases, it is supplied by a branch of the internal anterior thoracic, which reaches the muscle by passing over the small pectoral, across the triangular interval, to the under surface of the muscle about its centre. The *left* muscle divides into two portions, the outer of which, the larger, is flat and ribbon-shaped, passes down over the triangular interval between the two portions of the pectoral muscle on left side, and is inserted into the third costal cartilage. The inner portion arises from the common origin by a flat tendon, but has an additional attachment to the second piece of the sternum; it continues down, over, and to the left side of the sternum, forming a fusiform-shaped muscle, and ends by dividing into two aponeurotic slips, one of which is inserted into the lower end of the sternum, and the other into the fascia covering the pectoralis major. The nerve-supply, as in the others, is furnished by a branch from the internal anterior thoracic; from this branch another is given off which supplies the lower segment of the pectoralis major.

The pectoralis major muscle of each side, as mentioned above, is deficient opposite the second and third costal cartilages; the space is larger in the right than the left side, and, on both sides, is partially covered by the abnormal muscles. In the triangular interval of each side can be seen the nerve which supplies the m. sternalis. In this subject, also, on the right side, the platysma myoides is strongly developed, and passes for some distance below the clavicle. It is separated from the m. sternalis of that side by fascia and a thick layer of adipose tissue, and is on a plane quite superficial to the m. sternalis.

Note.—In adults I have only seen the m. sternalis three times[a] in 300 subjects. Some cases, no doubt, may have escaped my notice

[a] See Brooklyn Annals of Anatomy and Surgery, 1881–3.

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owing to the majority of the subjects having been injected through the heart, which necessitated a longitudinal division of the sternum. In all three cases the muscle was well developed. In one case it was continuous above with the opposite sterno-mastoid, and below was attached to the cartilage of the fifth rib. In the second case the muscle arose from the second costal cartilage, passed down over the pectoral muscle, and ended in an aponeurosis, which blended with the pectoral fascia. Some of the upper fibres intermingled, and were, apparently, continuous with those of the platysma myoides. The subject was a very thin one, and there was no adipose tissue over the chest. In the third case the muscle was attached, above and below, to the fascia covering the great pectoral. All three muscles occurred in males; two were in the left, and one on the right side.

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TWO CASES OF AN ABNORMAL CORONARY ARTERY OF THE HEART, ARISING FROM THE PULMONARY ARTERY, WITH SOME REMARKS UPON THE EFFECT OF THIS ANOMALY IN PRODUCING CIRSOID DILATATION OF THE VESSELS.

By H. ST. JOHN BROOKS, M.B.;
Demonstrator of Anatomy in Trinity College, Dublin.

[Read in the Sub-Section of Anatomy and Physiology, February 19, 1885.]

During the course of last Winter Session two very remarkable instances of a coronary artery of the heart springing from the pulmonary artery occurred in the Practical Anatomy Rooms of this University. The apparent rarity of this abnormality and the curious effect which it has in producing a cirsoid dilatation of the vessels in connection with the anomalous arteries have induced me to put on record the two cases in question.

In the first case that was discovered the anomalous coronary artery was a vessel which presented a calibre corresponding in size to that of a crow-quill. It sprang from the right anterior sinus of Valsalva of the pulmonary artery and passed downwards upon the infundibulum of the right ventricle, upon which it broke up into branches. These could be traced downwards for some distance upon the cardiac wall, and several of them were observed to anastomose with the branches of the aortic coronary arteries. In addition, however, to its cardiac twigs others of smaller size could be detected ramifying in the coats of the pulmonary artery. The walls of this abnormal branch of the pulmonary artery were exceedingly thin, and presented characters such as one would expect to find in a vein. On slitting up the pulmonary artery its orifice was observed to be placed in a manner identical to the corresponding openings in the aorta—viz., just immediately above the free margin of the semilunar segment of the valve.

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A consideration of this case will show that a very interesting question is connected with it. Here are two arteries belonging to the different circulations—the pulmonary and the systemic—anastomosing with each other. In these circulations, as is well known, the arterial pressure is very much greater in the systemic than in the pulmonary. How, then, did the blood flow in the anomalous coronary artery? There cannot be a doubt that it acted very much after the manner of a vein, and that blood flowed through it towards the pulmonary artery and from thence into the lungs.

The subject in which the second anomalous coronary artery was found was an elderly male. The vessel had precisely the same origin—viz., from the right anterior sinus of Valsalva—but it was very much larger, presenting a calibre quite equal to that of the aortic coronary arteries, which were in this case of unusually large size and very atheromatous. As in the preceding case, the origin of this vessel was placed beyond all doubt by slitting up the pulmonary artery and finding the orifice immediately above the corresponding semilunar valvular segment. So far as could be made out it gave no branches to the heart, but passed for a short distance to the left and then entered a complicated mass of tortuous, thin-walled, dilated vessels, not unlike a cirsoid aneurysm. This arterial mass was placed more or less around the pulmonary artery, lying chiefly behind and to the left of it, whilst a prolongation passed upwards upon the trachea and behind the aortic arch. The arteries composing it were bound together with dense connective tissue, so that it was exceedingly difficult to unravel them. Three large arteries (in addition to the pulmonary branch) entered this curious arterial arrangement; of these, one came from the right aortic coronary artery and passed behind both the aorta and the pulmonary artery to reach its destination; the second was a branch of the left subclavian, and it arose from this vessel in the root of the neck, close to the inner margin of the scalenus anticus, and descended in front of the aortic arch and pulmonary artery to take part in the formation of the cirsoid mass; the third branch took its origin from the posterior aspect of the transverse portion of the aortic arch and immediately entered the arterial mass. These three channels were of nearly equal size, presenting a calibre rather greater than that of a crow-quill. They were all abnormal branches, and the question comes to be—How have they

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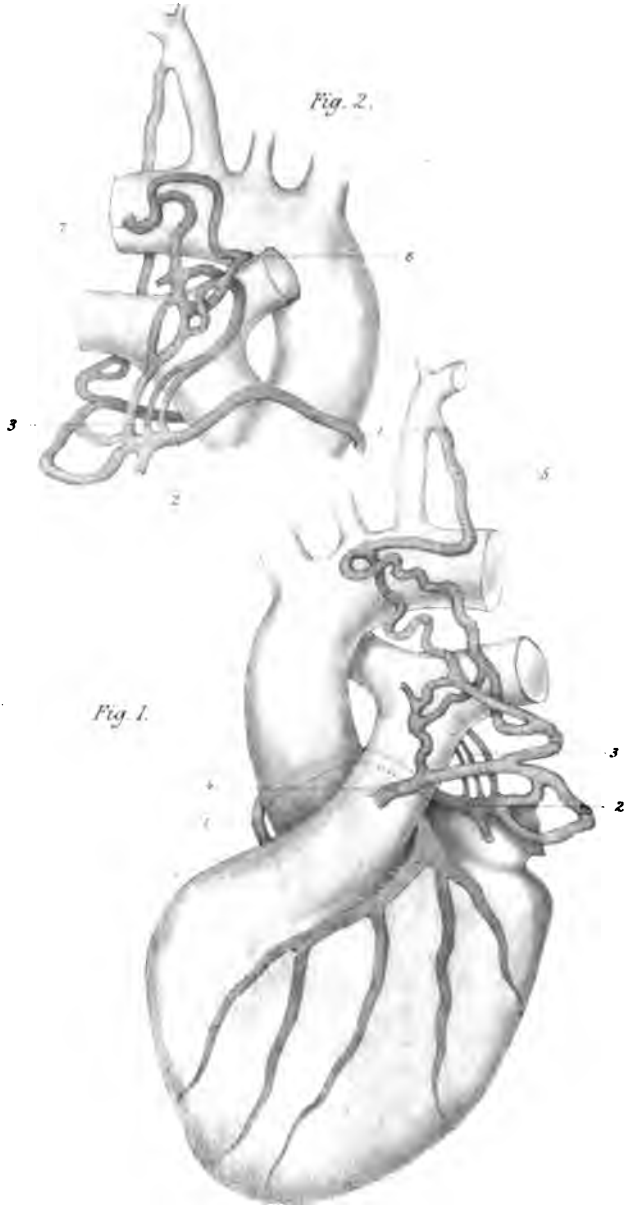

Dr St. J. Brooks on anomalous coronary arteries.

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sprung into existence? There can be little doubt that the whole arrangement of dilated tortuous vessels has arisen from a condition similar to that detailed in the first case—viz., from an anastomosis between a pulmonary and an aortic coronary artery. The double arterial pressure upon the walls of these vessels has led not only to their own dilatation but has opened up channels which, under ordinary circumstances, are invisible. These extra channels are probably formed out of certain of the minute arteries which are given off for the supply of the coats of the greater vessels. Why the tortuosity and dilatation should have occurred in the one case and not in the other it is impossible to say. It is likely, however, that the inosculations in the second case had been originally of a more direct and larger kind.

From the cirsoid mass several arteries ran upwards upon the trachea, and were evidently concerned in the supply of its wall, whilst one was traced into the lung of the right side upon the back of the bronchial tube.

The problem as to how the blood flowed in this arrangement is one of great difficulty. The three systemic arteries (viz., from the aorta, subclavian, and right coronary), doubtless, conveyed the blood into it; in all probability the pulmonary coronary branch acted as the channel by which the blood was drained away. A small quantity of the blood, however, was utilised for the supply of the trachea and the right lung.

W. Krause (quoted by Henle) records a very similar case to the first of those described in this paper. In his case, however, the anomalous branch came from the left anterior sinus of Valsalva of the pulmonary artery.[a]

EXPLANATION OF PLATE.

(1) Branch from right coronary, about ½ inch from its origin, passing behind aorta and pulmonary artery (indicated by dotted line), reappears at (2), where it gives off three branches (3) and anastomoses with an anomalous coronary branch (4) which arises from the right anterior sinus of Valsalva of the pulmonary artery. From this anastomosis two branches ascend in front of the bifurcation of pulmonary artery and transverse portion of arch of aorta, and join anomalous branch (5) arising from the subclavian artery near the origin of the vertebral.

The three branches (3) ascend, forming a cirsoid anastomosis between pulmonary artery and trachea, give off (6) a branch to right bronchus, and join anomalous branch (7) arising from aorta.

[a] Zeitschr. f. rat. Med. 1865. XXIV.

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VENA AZYGOS MAJOR ASCENDING ON THE LEFT SIDE, AND THE BEARING OF THIS ANOMALY ON THE DEVELOPMENT OF THE AZYGOS BLOOD-VESSELS.

By H. ST. JOHN BROOKS, M.B.;
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[Read in the Sub-section of Anatomy and Physiology, Feb. 19, 1885.]

The anomaly which is here described occurred in a male subject in the dissecting room of Trinity College this year. The dissectors of the abdomen had cut away the portions of the venæ azygos in the lumbar region before the aberrant condition of the thoracic portions of these veins was discovered. The lowest portion of the left azygos which remained uncut was lying on the body of the eleventh dorsal vertebra; from this point it bent towards the left, entering the eleventh intercostal space and receiving a tributary which crossed the head of the twelfth rib, and was formed by the confluence of three small veins in the lumbar region. It then ran upwards, receiving the eleventh and tenth intercostal veins, and, above the level of the tenth rib, bent inwards to reach the bodies of the vertebræ; it then ran vertically upwards, lying on the left side of the vertebral bodies, close to the aorta, receiving the remaining intercostal veins of the left side with the exception of the first and second. At the upper border of the fourth dorsal vertebra it bent upwards, forwards, and inwards, crossing the junction of the descending and transverse portions of the aortic arch, and the root of the left subclavian artery, to open into the left innominate vein. At the level of the body of the eighth dorsal vertebra, it was joined on the right side by a vein which divided into an ascending and a descending branch. Both these branches lay in front and rather to the right side of the bodies of the vertebræ; the ascending branch

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received the sixth, seventh, and eighth intercostal veins; the descending collected the blood from the ninth, tenth, and eleventh intercostal spaces; below this it was cut. There was a second vein in the sixth intercostal space. This ran upwards and, receiving the fifth, fourth, and third veins of the right side, formed a small trunk which crossed the necks of the ribs, lying external to the gangliated cord of the sympathetic; then, at the level of the third dorsal vertebra it bent forwards, arched over the root of the right lung and opened into the superior cava.

In a second case the course of the left azygos was similar, but it received no tributaries from the right side, and the right azygos (which was rather the larger of the two), took its normal course. This latter condition appears to be not very uncommon, but the first case—that of the greater part of the intercostal blood entering the left azygos—is much more exceptional; it is referred to by Marshall as "this somewhat rare condition," and both he and Henle quote but one case from Valentin—*Journ. de Méd.*, Paris, 1791.

It will be necessary, in order to appreciate the bearing of these anomalous conditions, to review briefly the development of the great anterior veins. In the embryo, before the formation of the vena cava inferior, two large trunks, the posterior cardinal veins, collect the blood from the intercostal spaces; and joining the anterior cardinals (primitive or external jugulars) form the right and left ducts of Cuvier. Afterwards, veins running parallel to the posterior cardinals, but internal to them on the bodies of the vertebræ (the posterior vertebral veins), are formed, and receive the intercostal veins, while the greater part of the posterior cardinals abort, the upper part, however, remaining, arching over the roots of the lungs to form right and left venæ azygos. The jugulars become united by a transverse branch (the future left innominate) and a stage is reached which is permanent in monotremes, and (in a slightly modified form) in some of the placental mammals, as in the rabbit. Further changes now take place by the abortion of the left duct of Cuvier, with the exception of its cardiac end, which forms the coronary sinus and the oblique vein of Marshall. A communicating

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branch forms between the two azygos veins about the level of the ninth dorsal vertebra, and the left azygos system becomes broken up, the blood from the lower spaces entering the right azygos about the ninth space, and the blood from the upper spaces (fourth to eighth) by another cross branch (vena azygos minor superior); the left superior intercostal vein remaining as a persistent part of the left azygos and entering the left innominate.

Now it will be noticed that the second and more ordinary of the cases described above differs from the stage which is permanent in monotremes, in the fact of the normal abortion of the left duct of Cuvier having taken place, so that the left azygos enters the left innominate. The first case seems, at first sight, to be merely a transposition from right to left of the normal arrangement. Thus, we have venæ azygos minor superior and inferior (formed on the right side) uniting into a common trunk, and running to the *left* across the body of the eighth dorsal vertebra to join the azygos major, and we have a rather large superior intercostal vein collecting the blood from four spaces and arching over the root of the right lung. But what is the signification of the trunk of this vein lying on the ribs external to the sympathetic instead of on the bodies of the vertebræ? It appears to me to represent a much larger portion of the posterior cardinal vein persisting than usual; and the portion of the left azygos lying on the tenth and eleventh ribs is also probably a part of the posterior cardinal.

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NOTES ON SOME ANATOMICAL ANOMALIES.

By F. T. HEUSTON, M.D., M.Ch., F.R.C.S.;
Lecturer on Anatomy, and Registrar of the Carmichael College of Medicine, Dublin.

[Read in the Sub-Section of Anatomy and Physiology, May 7, 1885.]

In this paper I have departed from my original intention, which was to have mentioned, for statistical purposes, all the anomalies found in the Carmichael College Dissecting-room during the past Session, but on collecting my notes I found they were of too voluminous a nature to be included in one paper, so I have selected the following five examples of muscular and five examples of vascular anomalies:—

MUSCULAR.

I. *Female, No.* 6.—*Left upper extremity; biceps.*—A well-developed muscle, arising between the insertion of coraco-brachialis and the origin of the brachialis anticus, passing on the inner side of the biceps, to which it had a fascial attachment, and being finally inserted into the semilunar fascia of the biceps.

II. *Male, No.* 24.—*Right upper extremity.*—The biceps having arisen by an additional head, similar to the preceding, was also inserted into the semilunar fascia, which, being thus very strong, gave origin to two distinct sets of fibres—(1) an outer or superficial head to the flexor carpi radialis, which united with that muscle about two inches below its origin; (2) from the deep surface of this muscle and fascia another set of fibres passed to be inserted into the radial aspect of the pronator radii teres about its centre. The origin and insertion of both pronator radii teres and flexor carpi radialis were normal.

III. *Male, No.* 36.—*Left upper extremity.*—The biceps arose by four heads; the central pair of which represented the normal long and short heads; the external arising from the tendinous attachment of the deltoid, and slightly from the neighbouring bone,

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between the origins of the triceps and the external prolongation of the brachialis anticus, was inserted into the external border of the biceps close to its tendon. The internal head had the same origin as the additional head in the two former cases.

Remarks.—The biceps being one of the most variable muscles in the body (holding third place in this class), would seem to present no room for extra description, but as there existed a couple of points in the above cases which have not received attention, I thought they were worthy of notice. With regard to an extra head to the muscle, the frequency of occurrence is great, being found by Hallet once in every eight subjects; Wood, 18 in 175; Theile, 1 in 9; Macalister, 1 in 10. Hyrtl states that the usual additional head is an aberrant slip of the brachialis anticus, but from what I have observed, not alone in the above cases but in numerous other examples I have met, I must agree with Macalister in stating that this is not ordinarily the case. Macalister ("Muscular Anomalies in Human Anatomy") notes an additional origin of the pronator radii teres from the biceps tendon and semilunar fascia, as also an origin of the flexor carpi radialis from same source; but I can find no record of a double origin as found in my second case, which, combined with the triple origin of the biceps, renders it of interest. Four heads to the muscle are much rarer, Macalister noting 2, Theile 1, Henle 1, Meckel 1, and Wood 1, which last is the only example with an origin similar to my case.

IV. *Female, No.* 21.—*Right lower extremity.*—A well-developed muscle arose from the lower fourth of the postero-internal surface and posterior border of the shaft of the fibula, and slightly from the malleolus; it soon terminated in a tendon, which passed through the internal calcaneo-malleolar grove, to be inserted in the inner border of the flexor accessorius and fibrous connexion between the tendons of the flexor longus digitorum and flexor longus pollicis.

V.—In the same subject another well-developed muscle arose from the posterior surface and internal border of the tibia for its lower fourth, the tendon passing through the internal calcaneo-malleolar grove, to be inserted into the internal border of the

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flexor accessorius. In both of the foregoing cases the tendon of the flexor brevis digitorum to the fifth toe was absent.

Remarks.—Although "Quain's Anatomy," 9th edition, states the above muscle (flexor accessorius longus digitorum) to be of frequent occurrence, I cannot find any satisfactory accounts as to the exact relations of the muscle to the flexor longus digitorum, particularly as to its usual insertion and frequency of occurrence; and I considered this case as being particularly interesting, showing in the one subject examples of the different origins described—viz., from fibula in the one extremity and tibia in the other, with practically the same insertion in both of the extremities. The symmetrical absence of the fourth tendon of the flexor brevis digitorum in this case was also interesting.

VASCULAR.

I. *Female, No.* 5.—On the left side the foramen spinosum was absent, while on the right side a small foramen existed, through which a minute artery passed. The place of the middle meningeal artery was taken on both sides by a branch of the ophthalmic artery, which, having its origin within the orbit, passed through the superior and external angle of the sphenoidal fissure, to be distributed after the manner of a normal middle meningeal artery.

II. *Female, No.* 25.—*Right upper extremity.*—An aberrant artery, which arose from the axillary, above the origin of its subscapular branch, passed in the arm to the inner side of the brachial artery, to unite with the radial about two inches below its commencement. In this case the brachial artery bifurcated about one inch above its normal position.

III. *Male, No.* 28.—*Right upper extremity.*—In this case also an artery arose from the axillary, opposite the upper border of the teres major muscle, and passing between the heads of the median nerve, proceeded down on the outer side of the brachial artery, to unite with the radial recurrent, which was of large size in the antecubital fossa.

IV. *Female, No.* 25.—*Left upper extremity.*—The suprascapular artery, arising from the axillary opposite the second rib, passed upwards beneath the clavicle through the cords of the brachial plexus,

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and having given a branch to the subscapularis muscle, passed beneath the transverse ligament with the suprascapular nerve, to be distributed as usual.

V. *Female, No.* 84.—*Right upper extremity.*—The ulnar artery, arising from the brachial at its usual place of origin, passed superficial to the flexor muscles of the forearm, giving off no important branch. The radial artery, while in the antecubital fossa, gave off a large branch, from which the following vessels had their origin—viz., anterior and posterior ulnar recurrent, radial recurrent, comes nervi mediani, and anterior and posterior interosseous, all of which had a normal distribution.

Remarks.—With reference to the foregoing, No. I. is stated by Quain as having been noted, although he does not state that the origin of the vessel was within the orbit.

Aberrant arteries are comparatively common, having been noted by Harrison in the proportion of three out of forty cases. Quain mentions nine examples out of 1,040 subjects examined. The most interesting points in those I bring under your notice are to be found in No. III., where the artery passed between the heads of the median nerve, and terminated in the radial recurrent artery. Quain mentions having seen an example of termination in the radial recurrent, which, however, took its origin from the ulnar artery. Monro and Meckel give examples of termination in the ulnar artery.

The fourth of my series (abnormal suprascapular) has been seen by Quain in three cases, but he does not state the relation borne by the vessels to the brachial plexus or to the supra-scapular ligament.

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A HERMAPHRODITIC GOAT.

By D. J. CUNNINGHAM, M.D., F.R.C.S.;
Professor of Anatomy in the University of Dublin.

[Read in the Sub-Section of Anatomy and Physiology, May 7, 1885.]

Numerous cases of so-called hermaphroditism in the goat have been recorded, many of which present features very similar to those of the specimen which I have the honour of bringing under the notice of the Sub-Section this evening. Still the condition occurs with sufficient rarity to justify me, I think, in calling your attention to it.

A few months ago a goat was brought to me for inspection by a Dublin dairyman. He was in doubt as to its sex, and he was desirous of obtaining my assistance in determining this point.

It was too young to present the general characters of either sex, but if anything it possessed more the build of a male. On examining the external organs of generation, a round aperture large enough to admit a goose quill was visible, about an inch in front of the anus; from this a shallow groove, floored with mucous membrane, passed forwards for a short distance. The integumentary folds which bounded this groove gained in prominence as they were traced forwards, and anteriorly they became continuous. Projecting out from the fore-part of the gutter there was a short, clitoris-like body, barely an inch long, and surmounted by an imperforate glans provided with a distinct prepuce. Whilst examining the animal, a practical demonstration was afforded of its mode of micturition. The urine flowed from the aperture in front of the anus. Lastly, there was a rudimentary scrotum, in which a solid oval body could be felt.

This being all that could be made out by external examination, the question arose:—Is this simply a case of hypospadias, or is it

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something more? Have we here a combination of male and female organs? Being unable to arrive at a conclusion regarding this, I purchased the animal with the intention of watching its growth, and afterwards its behaviour when brought into contact with the different sexes of its own species. As it grew it became decidedly male in its tendencies and habit of body, and as it was fast approaching adult life I determined to have it killed.

On the abdomen being opened I found:—(1) A large bicornuous uterus; (2) a capacious vagina; (3) two testicles placed between the two layers of the broad ligament. There were no traces of ovaries, and the right testicle had descended into the imperfect scrotum, and had drawn down with it the corresponding uterine horn.

Uterus.—The size of the uterus was very much the same as what we would expect to find in a female goat of the same age. The following were its dimensions:—(1) Length of body, 60 mm.; (2) length of right horn, 90 mm.; (3) length of left horn, 50 mm.

The right horn, which had been pulled into the scrotum by the corresponding testicle, was, therefore, very much longer than the left. But it was not nearly so roomy—it presented the form of a long drawn-out tube. Each cornu passed outwards between the two layers of the broad ligament, until it reached the globus minor of the testicle, and here it came to an abrupt ending by a blind extremity. Not the slightest trace of a Fallopian tube could be discovered, and the perfect closure of each horn was ascertained by filling it with mercury, when it was seen that no outflow whatever took place.

The cavity of the body of the uterus was very imperfectly mapped off from the vagina. A posterior constriction was the only external marking that showed where the one ended and the other began. The mucous membrane lining each chamber, however, was very different, and presented the usual uterine and vaginal characteristics.

The Vagina was 60 mm. long and very capacious. Contracting very much towards its lower end, it opened into the uro-genital

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canal, which in turn opened on the surface by the aperture already mentioned.

Testicles.—Each testicle was placed at the extremity of the corresponding uterine horn between the two layers of the broad ligament. The right testicle had descended into the scrotum, whilst the left remained within the abdominal cavity. They were both well developed, and showed the usual parts—even the hydatid of Morgagni.

The *vas deferens* ran along the under aspect of the uterine horn, and presented a very minute calibre, until it reached the body of the uterus. Here it suddenly expanded, and burying itself in the uterine and vaginal walls, it was carried downwards (like the duct of Gaertner in the sow), to open into the upper part of the urogenital sinus.

Vesiculæ Seminales.—Rudimentary vesiculæ seminales were likewise present. They appeared in the form of two or three lobular projections partially embedded in the vaginal wall, and they were, so far as could be made out, quite unconnected with the vasa deferentia.

Organ of Giraldé.—This organ, as is usual in the male goat, was well marked. Indeed, in this case it attained a length on each side of fully 2½ inches. It was composed of convoluted thin-walled tubes, coiled together so as to form a consistent mass of an elliptical form. By one extremity it was connected with the globus major and the body of the testicle, a direct tubular continuity existing between them. This was demonstrated by means of mercury injection, which flowed freely from the organ of Giraldé into the testicle. Some thin-walled tubes passed from the organ of Giraldé into the broad ligament, and ran along the uterine horn, but their mode of ending could not be ascertained.

These connections are of interest with reference to the history of the Wolffian body, of which this structure is a remnant.

From the foregoing description it will be seen that this goat, according to Simpson's classification[a] of hermaphroditism, belongs to the *double variety of true hermaphrodites*, presenting, as it

[a] Encyclopædia Anat. and Phys.

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does, testicles with combined male and female passages. This classification, however, cannot be considered a good one. The true distinguishing character of sex is the structure of the genital gland. The animal under our notice must, therefore, be regarded as a male with hypospadias and a concomitant enlargement of the uterus masculinus.

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